NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 57. Vol. IX. NOVEMBER, 1896.

NOTES AND COMMENTS.

To Collectors of Fossils.

DR. ARTHUR ROWE'S collection of Chalk fossils has long been known to those interested in the palæontology of that formation, not only for its large size, but for the loving care with which the specimens have been worked out. We are, therefore, glad to have persuaded its owner to furnish us with the account of his methods of preparation which is published in this number. Like a wise man, he has confined himself strictly to his personal experience, but it is clear that many of his suggestions and many of the methods which he employs for the fossils of the Chalk are applicable, with or without modification, to fossils from other formations; indeed, we understand that more than one private worker in other rocks has used the dental-engine with much success.

We draw attention to this paper because it hardly seems to be recognised by many of those who describe fossils even at the present day how necessary it is that their specimens should be properly cleaned, while it is not an exaggeration to say that more than half of the descriptive palæontology published by the past generation has to be done over again in consequence of the new evidence that springs to light, not by the discovery of new specimens, but merely by the proper cleaning of the old ones. A modern worker who endeavours to make a geological specimen tell him all that it has to say is often appalled by the perfunctory manner in which the palæontologists of a former age seemed to think it sufficient to do their work. Dr. Rowe's paper may also be perused with profit by the curators of many of our public museums, even, as he hints, by the officials of our Government establishments; for it is an undoubted, though a lamentable, fact, that there are displayed for the delectation of the British public hundreds of specimens which in their present condition are almost useless to the scientific investigator, but whose obscuring matrix might often be removed by five minutes with the dental-engine.

Dr. Rowe further complains that in most of our public and

private collections a very large number of the specimens are from unknown locality, while scarcely one has any indication of the zone from which it came. This has long been a complaint for the fossils from other formations besides the Chalk. We know well enough that foreign geologists are chary of exchanging specimens with British workers, because the collections they obtain lose more than half their value by being devoid of these details of locality and horizon. There has just been reappointed a Committee of the British Association for the purpose of determining the zones of the Carboniferous Limestone, and the first task that this committee will have to perform will be to persuade the numerous collectors up and down the country to pay a little more attention to these details, even should they be the rough-and-ready geologists of the old school who regard such minute modern methods as another sign of the degeneration of the times. But it seems to us that similar committees are just as much required for the other strata of our island. The Chalk itself, whose equivalents on the Continent are known by so many names and are split into so many subdivisions, is still in the minds of most of our collectors capable of no other division than into Upper and Lower Chalk. Any committee appointed for the purpose would naturally work through the medium of local societies, instructing them in the methods of zonal collecting by means of peripatetic lectures, and prevailing upon energetic local geologists to organise the collectors of their neighbourhood. Another line of action that ought not to be unprofitable would be to impress upon those in charge of our museums that at the present day it is of more importance to preserve imperfect specimens carefully localised than it is to go on acquiring magnificent show-specimens too precious to be of use to anyone.

WHAT IS PALÆONTOLOGY?

"PALÆONTOGRAPHY," we have said, "is not always palæontology." What, then, is palæontology? It is the fashion nowadays to say that it is merely a branch of zoology, thus carrying to the extreme the saying of Huxley that "the only difference between a collection of fossils and one of recent animals is that one set has been dead somewhat longer than the other." This is the morphological aspect of palæontology. Then there is the geological aspect, which confines itself to the use of fossils as "medals of creation," each being regarded as a token to tell the geologist the age of the rock in which it occurs. Important though both these aspects are, still palæontology has its independent and wider scope. It is important to trace the migrations of former races from one part of the world to the other, thus throwing light on problems of geographical distribution, not only now but in past ages. It is important that we should make comparative studies of the faunas found in various rocks, with a view to determining the conditions of life that occurred during their

deposition. It is important to study the aggregations of species in the rocks, that we may perchance find bionomic connections between the species belonging to different genera, such as undoubtedly obtain in modern seas. These are lines of investigation that cannot be followed in the museum alone, but that demand for their study long days spent in the field, carefully collecting and comparing the fossils from one horizon with those from another, carefully noting the positions of the organisms in the rocks and the mutual relations of associated specimens, and combining with this research the petrological and chemical examination of the matrices that contain the remains. All this constitutes a division of palæontology which is undoubtedly recognised by some of our best workers, but which the pure zoologist and the pure geologist are too often tempted to overlook, since it requires for its prosecution a careful training in both zoology and geology.

Pavingstone Palæontology.

It is, however, not our object to emphasise the need for the study of any particular branch of palæontology, but to point out the distinctness of faunistic study from that zoological and morphological division of the science, with which it is too often confused. The task of describing species and of investigating the structure of fossils must be handed over to men who have had a thorough training in the principles of systematic zoology and of comparative anatomy. It is useless, many of us think it is worse than useless, for the collector or even the geologist to attempt the taxonomic description of his own specimens: even when he does not make egregious mistakes, he is apt to be satisfied with a diagnosis that may possibly be enough for his brother hammer in the same parish working at the same restricted horizon, but that is of no value for the systematist approaching the subject from a more universal standpoint, and that, when placed before the morphologist, only succeeds in leading him astray. Even the mere determination of fossils has nowadays become, like the determination of recent animals and plants, a task for the specialist in taxonomy, and the lists that we constantly see issued in connection with stratigraphical papers can be proved, as Natural Science has before now proved some of them, to be lengthy aggregations of error. Correct conclusions cannot follow from such inaccurate premises; inevitably the determination, no less than the description, of fossils must be left to systematists, who should have had a training in general morphology, and who must be specialists on particular groups.

If the preceding remarks are as true as experience has led us to believe, it follows that the attempts of various most well-meaning persons to describe from a zoological standpoint the whole fauna or flora of a particular group of rocks are foredoomed to failure. Long ago Huxley scornfully stigmatised such attempts as "pavingstone

palæontology." It is the class of work that is turned out by these estimable and energetic people that has for so many years brought disrepute upon palæontology, with which, unfortunately, it became confused in the minds of zoologists. Sometimes, of course, these worthy folk have the wit to recognise their own ignorance, and by taking counsel with one friend after another have been able to produce compilations, the value of which, if not great, has at any rate been slightly increased. This, however, is unsatisfactory for science, and not altogether fair to the friends in question. Would it not be better if these would-be universal geniuses placed the whole work of zoological investigation in the hands of the zoological specialists? After they had done this, they need not complain that their occupation was gone, for there would still be much for them to do in the other division of palæontology to which we have alluded. They could still collect with care, still determine horizons and study associations of fossils, and, if they wished to compare the fauna existing at one horizon in one country with that existing at another horizon, or in another country, they would be able to do so with far better results, since the materials for discussion would no longer be incorrect determinations and imperfect descriptions, but the carefully elaborated conclusions of many highly-trained specialists.

A Plea for Detail in Stratigraphy.

The preceding notes had been written before Mr. J. E. Marr delivered his address to the Geological Section of the British Association, and we were delighted to find similar views forcibly urged by so recognised an authority. Mr. Marr occupied the main part of his address "with reasons for the need of conducting stratigraphical work with minute accuracy." He did well, for there are in this country many people, professing to speak as geologists and palæontologists, who still pooh-pooh the detailed investigation of fossils and the correlation of minute subdivisions of stratified systems with one another. They say that in such work we lose sight of broad generalisations, that correlation is absurd when species must have taken time to migrate, that the field-geologist really cannot map so trivial a thing as a "zone," much less a "hemera," that the poor student is getting so puzzled he cannot pass his examinations. The absurdity of such protests was temperately yet thoroughly exposed by Mr. Marr. It was the minute palæontological investigation of strata that unlocked the secret of the Highlands and inaugurated a new era in physical and dynamic geology; microscopic examination of sediments, coupled with accurate zoological knowledge, has thrown brighter light on the problem of the permanence of oceans than all the speculations of the physicist, whose "imposing superstructure of mathematical reasoning" is "often founded upon very imperfect data"; the history of volcanic action, the reconstruction of former

geographies, the explanation of the present contours of the earth, all these are elucidated by, indeed are impossible without, the most careful work of the stratigrapher. As for the influence that this kind of work has on our knowledge of ancient life, we need not repeat what we have already said; but the following passage expresses so forcibly a truth for which we are always contending that we quote it as fully as our space permits :—

"The importance of detailed observation in the field is becoming every day more apparent, and the specialist who remains in his museum examining the collections amassed by the labours of others, and never notes the mode of occurrence of fossils in the strata, will perhaps soon be extinct himself . . . In the first place, such a worker can never grasp the true significance of the changes wrought on fossil relics after they have become entombed in the strata, especially amongst those rocks which have been subjected to profound earth-movements; and it is to be feared that many 'species' are still retained in our fossil lists whose supposed specific characters are due to distortion by pressure. But a point of greater importance is that one who confines his attention to museums cannot, unless the information supplied to him be very full, distinguish the differences between fossils which are variations from a contemporaneous dominant form, such as 'sports,' and those which have been termed 'mutations,' which existed at a later period than the forms which they resemble. The value of the latter, to those who are attempting to work out phylogenies is obvious, and their nature can only be determined as the result of very laborious and accurate field work; but such labour in such a cause is well worth performing. The student of phylogeny has had sufficient warning of the dangers which beset his path, from an inspection of the various phylogenetic trees, constructed mainly after study of existing beings only . . . but recent researches amongst various groups of fossil organisms have further illustrated the danger of theorising upon insufficient data, especially suggestive being the discovery of closely similar forms which were formerly considered to be much more nearly related than now proves to be the case. . . . As the result of careful work, dangers of the nature here suggested will be avoided, and our chances of indicating lines of descent correctly will be much increased. It must be remembered that however plausible the lines of descent indicated by students of recent forms may be, the actual links in the chains can only be discovered by examination of the rocks, and it is greatly to be desired that more of our geologists, who have had a thorough training in the field, should receive in addition one as thorough in the zoological laboratory. Shall I be forgiven if I venture on the opinion that a certain suspicion which some of my zoological fellow countrymen have of geological methods is due to their comparative ignorance of palæontology, and that it is as important for them to obtain some knowledge of the principles of geology as it is for the stratigraphical palæontologist to study the soft parts of creatures whose relatives he finds in the stratified rocks?"

For these and many other pointed remarks, all those who are endeavouring to do work of real value, however minute their share, owe the president of Section C a debt of gratitude, and they will join with us in the hope that his Address will find its way into many a dim recess that they know only too well.

THE ORGANISATION OF SECTIONS AT THE BRITISH ASSOCIATION.

WE do not know whether it was its President's eloquent plea for minute detail that affected the work of the Geological Section at Liverpool, but certainly it was as detailed, and consequently as dry, as the most Precambrian of fossils could wish. The ordinary person does not feel strongly impelled to hear a disquisition on "Two Examples of Current Bedding in Clay," or on "Quartzite Lenticles in the Schists of S.E. Anglesey." Even certain papers that did attract us by their titles, such as "The Depths of the Sea in Past Epochs" and "The Conditions under which the Upper Chalk was deposited," proved when we heard them to be for the most part repetitions of matter that we had already seen in print many years ago, and that, if their authors escape injury, we do not despair of seeing in print again as many years hence. But we should not like it to be inferred that other Sections were much better. "Fever in Mice" did, it is true, attract the ladies to the Physiological Section, only to disappoint them with long statistics of temperature. No one, however, can have been drawn to hear of the Action of Pilocarpine on the Eosinophile Granules of the Leucocytes, in the same Section; or, except for the sake of securing a good place to hear Mr. Francis Darwin later in the day, can many have been induced to attend the Botanical Section at 10.30 to be told about the Arrangement of the Vascular Bundles in certain Nymphæaceæ.

Of course, the wise man does not go to hear these papers: he knows that many of them are read for the sake of self-advertisement, and that any which are of value will soon be accessible through the ordinary channels of publication. For all that, it seems well to protest, if only in the hope that our words may reach the Canadian cousins who are preparing so splendid a welcome for the Association next year. These papers not merely clog the work of the Sections, but they have an effect directly contrary to the main object of the Association. Some, perhaps, read by local naturalists, or descriptive of local phenomena to which the attention of visitors should be directed, are not to be discouraged, but the rest both frighten away the people whom the Association wishes to attract, and, what we feel to be of more importance, tend to split the scientific visitors themselves still further among sections. The Association should rise above the specialism of most of our learned societies; it should offer a field where the zoologist might confer with the botanist, where both might exchange experiences with the geologist, and where all three might pick up something of use to them from the physicist and chemist, who in their turn need not go the poorer away. Let there be more discussions on matters of general interest, and let them be thrown open to even more sections. Why, for instance, should the discussion on Neo-Lamarckism have been confined to zoologists? Surely some of the physical problems that were hinted at in the discussion on the cell might have been laid before the physicists.

There are some who decry these general discussions, who say that the good men will not speak, that no one keeps to the point, that they only appeal to the Association bore. In this there is much truth, but it only proves that better organisation is needed. The subjects of discussion should be more strictly limited, and speakers should be kept to the point. We mentioned a "Discussion on the Cell"; why, one might discuss the cell for a month. But, as our Note on it shows, the discussion was almost entirely confined to the mechanics of nuclear movement, and a speaker who discussed cell-division on broader lines was told he was straying from the point. The truth was that there was no point to stray from. Again, instead of a discussion on Neo-Lamarckism, which was crushed out by the very vastness of the subject, or one on early man in the Mediterranean, which elicited little besides a string of contradictory statements, why could we not have had some definite question clearly put before us, or some definite thesis maintained? The organisers should get people with knowledge to attack the proposed problem from definite sides, and should explain to them beforehand precisely what the subject of discussion was to be. Even the public need not be snubbed or misled, and an abstract of the opener's speech might be issued the day before.

Again, if we must have sections meeting separately, we should like to see the sections dealing with what we call the natural sciences housed a little nearer to one another. At Liverpool, geology was three-quarters of a mile away from zoology and from the temporary museum, where many specimens of great geological interest were exhibited. Of course, there are often mechanical difficulties, but we imagine that the divorce is occasionally due to misunderstanding quite as much as to incompatibility.

Modification and Variation.

It must be admitted, however, that the Zoological Section did its duty in the way of discussion of general principles, both by itself and in conjunction with other sections. The first discussion of importance was that which purported to be on Neo-Lamarckism, and which was opened by Professor Lloyd Morgan. In a lucid paper he brought forward his useful distinction between *variations*, which are of germinal origin and congenital, and *modifications*, which are impressed on the organism by its environment. The question at issue between the Neo-Darwinian and the Neo-Lamarckian has of course been whether modifications, which are admitted by all, can be transmitted from parent to offspring, and so come under the influence of natural selection in the same way as variations are transmitted and come under that influence. Professor Lloyd Morgan, while theoretically holding the strict Neo-Darwinian position, that modifications are non-transmissible, considers that they may not be

without their effect upon the evolution of the race, since through them environment may act upon variation indirectly. In a word, he supposes that an existing modification opens a way for the occurrence of a similar variation. The modification is, *ex hypothesi*, in accord with the environment, and its existence in the parent permits of variation in a similar direction in the offspring. He illustrated this by comparing the tendency to variation to the swinging of a number of pendulums in all directions, while the action of the environment or the action of the existing structure of the parent acts as a check on the pendulum swing in a certain direction, though permitting it in those directions that are harmonious with the existing structure. Thus any further modification of parental structure opens a new plane for the swing of a new pendulum.

Unfortunately the subsequent speakers did not, for the most part, direct their remarks toward this thesis of Mr. Lloyd Morgan's. We shall therefore confine ourselves to urging the following difficulty. The modification induced by environment arises necessarily after the fertilisation of the ovum, and in most cases a considerable time after that event; variation, on the other hand, either is inherent in the ovum or spermatozoon, or is a result of the process of fertilisation. In the life of a single individual it is obvious that no modification can affect variation, since this is necessarily antecedent. Again, to suppose that a modification of either the male or the female parent affects potential variation in the spermatozoon or the ovum, is merely to re-state, in other words, that a change of environment which affects the parent likewise affects the generative products; and such a statement is nothing more or less than Cope's theory of Diplogenesis, which already holds the field as one of the main theories of the Neo-Lamarckian.

ARE WE DESCENDED FROM KING-CRABS?

THE next important discussion in which the zoologists took part was that which, by an innovation in the Association's procedure, was permitted to follow the Address of the President of the Physiological Section. Dr. Gaskell took this opportunity of forcing on the attention of biologists the theory which he has previously put forward more than once, that the Vertebrata are descended from Arthropoda, the points of connection being represented among recent animals by *Ammocœtes*, the larva of the lamprey, on the one side, and by *Limulus*, the king-crab, on the other. It is certain that during the two hours in which Dr. Gaskell delivered his very remarkable address he was able to adduce a quite astonishing number of coincidences in structure, as shown in the annexed table; and it is a striking fact that his hypothesis should have led him to the discovery of structures previously unsuspected, such as the sense-organs in the flabellum of *Limulus* and the camerostome of *Thelyphonus*, which he claims as corresponding to the auditory and olfactory organs of *Ammocœtes*.

Table of Coincidences between Limulus and its Allies, and Ammocœtes and Vertebrates (after Gaskell).

Limulus and its Allies.	Ammocœtes and Vertebrates.
Central Nervous System.	
Supra-œsophageal ganglia ..	Cerebral hemispheres.
Optic part	Optic thalami, ganglia habenulæ, &c.
Olfactory part	Olfactory lobes.
Œsophageal commissures ..	Crura cerebri.
Infra-œsophageal ganglia.. ..	Epichordal brain.
Prosomatic ganglia	Hind brain, cerebellum, post-corp.quadrig.
Mesosomatic ganglia.. ..	Medulla oblongata.
Ventral chain.	
Metasomatic ganglia.. ..	Spinal cord.
Alimentary Canal.	
Cephalic stomach	Ventricular cavities of brain.
Straight intestine	Central canal of spinal cord.
Terminal part..	Neurenteric canal.
Œsophagus..	Infundibular tube and saccus vasculosus.
Mouth tube..	Hypophysial tube, later nasal canal.
Liver	Part of subarachnoideal glandular tissue.
Appendages and Appendage Nerves.	
Prosomatic or locomotor appendages	Appendages of oral chamber or stomatodæum.
Foremost appendages ..	Upper lip and tentacles.
Last appendages	Velar appendage and median ventral tentacle.
Metastoma	Lower lip.
Nerves of prosomatic appendages	Various branches of Vth nerve.
Mesosomatic or branchial appendages	Appendages of branchial chamber.
Opercular appendages ..	Appendage innervated by VIIth nerve.
Genital part	Thyroid gland & pseudo-branchial groove.
Branch. part	Hyobranchial.
Basal part	Septum of stomatodæum.
Branchial appendages ..	Branchial appendages innervated by IXth and Xth nerves.
Special Sense Organs and Nerves.	
Lateral eyes and optic nerves ..	Lateral eyes and optic nerves.
Median eyes and nerves	Pineal eyes and nerves.
Camerostoma and olfactory nerves	Olfactory organ and Ist nerve.
Flabellum and nerve	Auditory organ and VIIIth nerve.
Epimeral nerves to surface of prosoma and mesosoma	Sensory part of Vth nerve.
Internal and External Skeleton.	
Internal skeleton.	
Branchial cartilages	Branchial cartilages.
Entapophysial cartilaginous ligaments	Subchordal cartilaginous ligaments.
Fibro-massive tissue (forerunner of cartilage or "Vorknorpel")	Muco-cartilage or "Vorknorpel."
External skeleton.	
Chitinous layer	Cuticular layer on surface of body and subepithelial laminated layer.
Excretory Organs and Cœlomic Cavities.	
Coxal gland..	Pituitary gland.
1st head cavity, præoral	1st head cavity, præoral.
2nd head cavity. Cavity of prosomatic segments	2nd head cavity, mandibular.
Cavities to each mesosomatic segment	Cavities of hyoid and branchial segments.
Heart and Vascular System.	
Dorsal heart	Column of fatty tissue dorsal to spinal cord.
Longitudinal venous sinuses ..	Heart and ventral aorta.
Lacunar blood spaces of appendages	Lacunar blood spaces in velar and branchial appendages.

Since, however, Dr. Gaskell's arguments have long been before the scientific public, and since his complete address can now be easily obtained and studied, we shall not recapitulate them here. It may, however, be interesting to record some of the objections that were raised in the discussion.

In the first place, it is interesting to note that, though one or two speakers, such as Dr. Gadow, seemed inclined to support Dr. Gaskell, or at least to demand for him a fair field, still no attempt was made by anyone, except Dr. Gaskell himself, to rebut the arguments brought forward by the other side.

As for the supposed difficulty in the tubular nature of the vertebrate central nerve-cord, and its lining by non-nervous tissue, Professor Weldon, comparing the development of the nervous system in worms, pointed out that the nervous tissue arose from the deeper layers of the derm, which were separated from the outside by a layer of non-nervous ectoderm, while, as Mr. Garstang also insisted, the conversion of the invertebrate system into a tubular system was to be traced in the collar of *Balanoglossus*, an animal which Dr. Gaskell ignored. Professor Minot, on the other hand, reminded us that the nervous system of Vertebrata was not embryonically a tube, but arose in each individual as a solid mass, which became hollowed afterwards; such a solid nervous system is also found in Invertebrata, and there are no grounds for denying the homology. As for the epithelial lining, the nervous system in Invertebrata has a different embryonic origin from the digestive tract, whereas in Vertebrata the lining of the nerve canal has the same embryonic origin as the central nervous system. The infundibulum, too, if it represented a mouth, should be at the end of this tube, whereas it occurs at the base of the ventral zone of the brain at some distance from the end of the supposed tube. The cerebral hemispheres, moreover, are a product of the dorsal zone of the brain, and therefore cannot be the simple homologues of the invertebrate supra-œsophageal ganglion.

Dr. Gaskell would explain the packing tissue of the *Ammocœtes* skull as derived from the liver of *Limulus*. But similar packing tissue is not so rare, for it occurs, as stated by Professor Weldon, surrounding the alimentary canal, ovaries, and liver of the crustacean *Nebalia*, and, as Professor Minot pointed out, round the supra-renal capsules of man, though not of rodents.

We believe that Professor Hickson was right in affirming the agreement of all morphologists that the alimentary canal of the Vertebrata is homologous with that of the Invertebrata. There may be dispute as to the precise ancestor, but as to this fundamental fact there is none. Dr. Gaskell, however, would have us believe that an entirely new alimentary canal was originated by the closing in of the *Limulus* appendages around a cavity on the ventral surface, portions of these appendages forming the vela of the proctodæum and stomodæum. These latter, unfortunately for the hypothesis, are

structures that occur not only in vertebrates but in invertebrates, and even in *Limulus* itself. The gill-slits in the anterior region of the alimentary canal are supposed by Dr. Gaskell to be the original spaces between the concresced limbs; why then, asked Mr. Weldon, are they formed in a vertebrate as slits gradually appearing in a continuous wall, and why is there no trace of them in the post-branchial region? More fundamental was the objection so clearly stated by Mr. MacBride that in the simplest type of vertebrate development with which we are acquainted (that of *Amphioxus*) the alimentary canal is formed by invagination, precisely as it is in the arthropod *Lucifer*, whereas the nerve-cord is formed much later by a simple layer of cells, over which a sheath of non-nervous cells grows as a protection.

Out of all these difficulties Dr. Gaskell extricates himself by denying the fundamental postulates of embryology, and by setting aside deep-seated homologies accepted by all comparative anatomists, whatever their peculiar heresies may be, in order to set up for himself a new method of morphological investigation, or rather, as he complacently informed us, a very antiquated one revived. The coincidences of adult structure on which he lays such stress are coincidences of a character familiar to those who study more than one group of animals, and are often of very superficial nature. It is, for instance, not to be wondered at that a transverse section of the thyroid should resemble that of the genital duct of a scorpion, for as Professor Weldon remarked, similar glandular arrangements are not so rare among invertebrates. Nor can much stress be laid on the presence of a chitinous layer in the skin of *Ammocœtes*, when we observe that this is not outside the epidermis, as it is in Arthropoda. As for the limbs of *Pterichthys* being morphologically comparable with those of *Limulus* or the Merostomata, this was stoutly denied by our chief authority on the subject, Dr. Traquair; while Mr. Bather, besides dwelling on the difficulty of the transmutation of a highly specialised type of structure into another highly specialised type with totally different physiological relations, insisted that, if this were really so, the absence of the well-developed connecting links, with their presumed chitinous exo-skeletons, from the record of the rocks was almost inconceivable in face of the fact that the broken ends of the supposed chain were so fully preserved to us in the Silurian and Devonian periods.

Finally, Mr. Hoyle summarised what evidently was the view of most zoologists present, in his parable of the medical student who, chancing on a cuttle-fish, wrote a learned and elaborate paper clearly demonstrating that every one of its structures was homologous with, and the immediate precursor of, the organs of a typical vertebrate.

Cell Facts and Cell Theories.

Yet another discussion, this time between zoologists and botanists (why not also physiologists?), was that on the Cell, initiated by

Professor Farmer, whose kindness has permitted a full abstract of his paper to be inserted among our articles. The discussion itself afforded examples of that disagreement among biologists to which Mr. Farmer alludes. Professor Zacharias was one of those who held that the radiating fibres seen in a dividing cell were in no sense contractile, but the expression of lines of force, and who rejected the individuality of the chromosomes. Dr. Gustav Mann, on the other hand, maintained the permanence of the centrosomes and the definiteness of their structure, while, on the ground of physiological evidence, such as that the chromatin in a gland-cell might change from day to day, he denied definiteness to the chromatin of the cell. The disagreement of doctors, if not the opposing evidence of acknowledged facts, is enough to show that we are not yet in a position to base elaborate hypotheses on such inconclusive evidence. While, therefore, we may admit with Professor Yves Delage that Weismann's theory is the only one that explains how the special characters of the male and female parent are contained in the fertilised egg, we may also agree with him that the theory is contrary to the evidence at present before us. We may even go so far as to maintain with Dr. C. S. Minot that the interest in the celebrated theories of "The Germ-plasm" is now a historical one. However this may be, there is one point on which all speakers were agreed, namely, that future advance will be made chiefly by the investigation of the living cell, through methods which were not at the disposition of the older observers.

"Anatomical Characters" in Systematic Botany.

In the Botanical Section, the presidential address, by Dr. D. H. Scott, put the present position of morphological botany in a clear light. At the outset he deplored the want of a study of the comparative embryology of seed-plants. It is often assumed that the development of the embryo from the fertilised ovum will teach us but little in botany; an opinion which is largely based on ignorance. As regards the value to systematists of characters which those gentlemen are pleased to call "anatomical," there is no doubt, as Dr. Scott himself has shown, that certain well-marked peculiarities of internal structure are constant in more or less definite systematic groups. Such a character is the presence of bast on the inside, as well as on the outside, of the wood-bundles in several orders of dicotyledons, *e.g.*, Solanaceæ, Asclepiadaceæ, Apocynaceæ. Similarly, Masters has shown that in many conifers anatomical characters may be useful for diagnosing species. In the pines, for instance, the position of the large resin-canals as seen in the transverse section of the leaves, the arrangement of the stomata, the branching or not of the foliar bundle, are all of some value. In the most recent revision of the Acanthaceæ the sculpture of the pollen-grains is extensively used as a generic

character by Lindau, while Hallier makes bristly-pollen the most important feature in his limitation of the genus *Ipomœa*.

Just as in zoology, there is a growing tendency among the younger systematists to make use of a far wider range of characters, the constancy of a character, and not its position, being the quality that gives it classificatory value. Where botany appears to differ from zoology is in the fact that so few changes are necessitated by the more rigorous tests; and this says much for the acumen of the older workers.

Alternation of Generations in Plants.

Where must one seek the origin of the second generation? In the mosses, ferns, and higher plants there is a regular alternation of a sexual and an asexual generation. In the Algæ and Fungi there is no such regularity; the same individual may bear reproductive organs of both kinds. Is the neutral or asexual generation (the sporophyte) something new which has been intercalated between two sexual stages (oophytes), the latter alone being comparable with the individual in the thallophyte? or is the sporophyte also comparable to such an individual, the two generations contrasting only in this—that in the one reproduction depends absolutely on a sexual process, in the other is purely asexual? In a word, is the alternation *antithetic* or *homologous*? Bower, the chief exponent of the former theory, conceives the sporophyte to have arisen as a mere group of spores in a fertilised ovum. Hence the origin of its vegetative portion, whether thallus or stem, leaf, and root, must be sought in the sterilisation of this sporogenous tissue, that is to say, in a conversion of spore-forming into vegetative cells. In the address we are discussing, Dr. Scott preferred Pringsheim's view, which, by regarding the two generations as homologous, obviates the necessity of a new birth. On this view, the sporophyte is simply an individual precisely comparable with the oophyte, but it has acquired the habit of producing only asexual spores, while the case of the oophyte is the converse. As is well known, many green Algæ, such as *Ulothrix* and *Œdogonium*, can form asexual spores (zoospores) at any stage of their development, but there is one stage, namely, on the germination of the fertilised ovum (oospore), at which they are always formed. There is no difference at all between the two sets of zoospores; there is no indication of anything new. In another green alga, *Sphæroplea*, zoospores are formed only on the germination of the oospore; that is to say, there is here as definite an alternation of a sexual generation (the *Sphæroplea* plant) and an asexual (the oospore with its zoospores) as in the mosses.

Mosses and Ferns.

Concerning the relations between mosses and ferns, Campbell's *Anthoceros*-like ancestor for the latter does not find favour with Dr.

Scott. "An unprejudiced comparison," says he, "seems to me to show nothing more here than a very remote parallelism, not suggestive of affinity." "There is no reason to believe that the Bryophyta, as we know them, were the precursors of the vascular Cryptogams at all." "Things seem to point to both Pteridophyta and Bryophyta having had their origin far back among some unknown tribes of Algæ." The utter hopelessness of attacking such a problem is very forcibly put. It is generally agreed that Gymnosperms arose somehow from the vascular Cryptogams. Yet in the far-off days of the Devonian we find both flourishing together. "Hence, in the Devonian epoch, there had already been time, not only for the Pteridophyta themselves to attain their full development, but for certain among them to become modified into complex Phanerogams. It would not be a rash assumption that the origin of the Pteridophyta took place as long before the period represented by the plant-bearing Devonian strata as that period is before our own day. Can we hope that a mystery buried so far back in the dumb past will be revealed?"

A Nesting Fish (*Amia calva*).

In his valuable account of the early development of *Amia calva* (*Quarterly Journal of Microscopical Science*, vol. xxxviii., p. 413), Dr. Bashford Dean describes the curious nesting habits of that archaic fish. In the beginning of spring the fish leave the deeper waters of the lake and sun themselves in the shallow, swampy ends, where Chara, Potamogeton, and bulrushes abound. Immediately before spawning the fish divide themselves into parties, each consisting of a female and several males. The nest is prepared some time before spawning. Its method of construction is doubtful: "fishermen state that the spawning party prepares it by constant circlings . . . . . the soft weeds and rootlets appear bent and brushed aside in a way that gives it somewhat the appearance of a crudely-finished bird's-nest." The spawning fish leaves the nest from time to time, returning in company, and the eggs and milt are shed simultaneously. The eggs, after being fertilised in the water, become instantly adhesive, sticking to the roots of the water-plants. The early stages of development are rapid, the larvæ all disappearing from the nest twenty-four hours after the eggs are laid.

A curious feature is that the male acts as guardian of the nest and caretaker of the young. Soon after all the eggs are laid, a single male takes his position on the nest, and his constant movements no doubt aid in keeping the eggs free from sediment, and duly supplied with oxygen. The larvæ leave the nest in a body, and presently appear as a dense swarm swimming under and around the male. The male remains with them for some time, gradually leading them away from the breeding ground, and during this period he is most zealous and courageous in their defence.

Bird Day.

A "Bird Day" for America, on the same lines as the "Arbor Day," was suggested some two years ago by Mr. C. A. Babcock, of Pennsylvania, and approved by the U.S. Department of Agriculture. Its observance has again been pressed upon the attention of the National Education Association, in a circular dated July 2, 1896, which points out that the object of the Government is "to diffuse knowledge concerning our native birds, and to arouse a more general interest in bird protection." It is also stated that "one of the greatest benefits of Arbor Day is the sentiment and interest aroused in the subject of trees and in the broader study of nature." As appeals to the humanity of woman are fruitless, and the ignorance of the farmer is still deplorable, perhaps the best means for the preservation of feathered beings is to interest the young in birds in general, stress being especially placed upon their more interesting habits, their foods, and uses. The circular fully recognises the harm done to agriculture by the "Scalp Act" of Pennsylvania, of 1885, and the uselessness of waging war against noxious animals, because it almost invariably leads to the wholesale destruction of life, and the destruction therefore of many animals of inestimable value to the agriculturalist.

Phosphatic Chalk.

A second locality for phosphatic chalk has been discovered by Mr. John Rhodes, fossil collector to the Geological Survey, and has been described by Mr. Aubrey Strahan in the *Quarterly Journal* of the Geological Society for August. The newly-discovered bed is from one to two feet thick, and is seen throughout the section in the Southerham Pit "on the south-western side of the high downs of Upper Chalk which overlook Lewes from the east." In composition the Southerham phosphatic chalk is almost identical with that of Taplow; the brown grains can easily be separated from the chalky matrix by a weak solution of acetic acid. Mr. Strahan refers the bed to the horizon of the Chalk Rock, and points out that in this respect alone it differs from the Taplow deposits, which occurred in the *Actinocamax quadratus* zone of the Upper Chalk. Fish-remains are abundant in the phosphatic band, and Mr. Chapman has given a list of forty-two Foraminifera and six Ostracoda in an appendix to the paper.

A very rich phosphate from Taplow, containing no less than 65 per cent. of phosphate of lime, was exhibited at the July meeting of the Geologists' Association by Mr. F. W. Rudler, on behalf of the discoverer, Mr. de Mercey.

English Cave Dwellers.

While the Irish Cave Fauna is being investigated by Messrs. G. H. Carpenter, H. Lyster Jameson and others, to whose work we

have often alluded (NATURAL SCIENCE, vol. vi., p. 148, March, 1895, and vol. viii., p. 371, June, 1896), the modern inhabitants of the caves of England are receiving the attention of Professor Denny, of Firth College, Sheffield, who is engaged with the exploration of the caves of Derbyshire. Although Mr. Denny has not yet published anything on his researches, we understand that the famous Peak Cavern at Castleton has already yielded a varied fauna, comprising representatives of the following groups of Invertebrata :—Chætopoda, Crustacea, Myriopoda (Diplopoda), Insecta (Diptera, Coleoptera, Collembola), Arachnoidea (Araneidæ and Acarina). Among these Professor Denny has found several blind types, but only one appears to be a true troglodyte, and that, strange to say, is none other than the Collembolan—*Lipura Wrightii* (of Carpenter)—which was, we believe, the first true cave animal discovered in the Mitchelstown Cave, of which we have heard so much of late. Mr. Martel (President of the Société Spéléologique of Paris) was therefore not quite accurate, in his recently published account of Mitchelstown Cave, in stating that "It is the only grotto in England, Scotland, or Ireland where, up to the present time, there have been found animals peculiar only to caverns."

THE delegates from local societies corresponding with the British Association unanimously resolved at the Liverpool meeting that Mr. G. Abbott's paper on District Unions of Natural History Societies, published in NATURAL SCIENCE for October, under the title of "The Organisation of Local Science," "be distributed by the Committee of Delegates amongst *all* the natural history societies in the United Kingdom," with the object of obtaining their opinions on the feasibility of the plan. Clearly, the delegates are more in favour of federation than they were when the proposal was first introduced at their meeting in Montreal, in 1884. It will be curious if Toronto proves the scene of its ultimate adoption.

Pressure on our space obliges us to hold over till December further notes with which our correspondents at the Liverpool meeting of the Association have favoured us. In our next number, too, we shall publish Mr. MacBride's paper on the position of Morphology, which gave rise to some discussion.

AN error, as annoying as it was obvious, crept into the Note, "Natural Science at Cambridge," in our October number. We said that "over three hundred unnamed specimens of Bryozoa" had been given by Miss E. C. Jelly to the University Museum of Zoology. It should, of course, have been "named specimens." Mr. S. F. Harmer, whom we thus misquoted, kindly informs us that some of the specimens have, in fact, "almost the value of type-specimens, since they belong to the same lots as those from which the original descriptions of the species were drawn up." We offer sincere apologies to Miss Jelly and Mr. Harmer.

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I.

The Influence of Mind in Evolution.

THAT Charles Darwin, intent on his own discovery, the influence of natural selection by the survival of the fittest, should have given less than their due to other causes that may have contributed to the evolution of the colours and forms of animals was natural enough. Perhaps it was equally natural that his more ardent followers should go further than himself, and see in natural selection the sufficient explanation of everything. The tendency towards sheer materialism, which is perhaps an inevitable first result of accepting Darwin's theory, has disinclined them even to allow to an animal's habits and actions any influence in modifying the form of the species. All animal life is to them a gambling-table, in which every number in turn comes up, but only those which chance to fall into the right hole, labelled "environment," count towards the winning of the game. This view reaches its climax in Weismann's doctrine that characters acquired by an individual after birth are not transmitted to its posterity. Weismann is a great name, but since Herbert Spencer has stood up against him as the champion of the opposite doctrine, that acquired characters are transmitted and that the use or disuse of any organ is one of the most potent among the influences that control its development, meaner men who have never been able to believe that natural selection does explain everything may take courage to admit their conviction. I am one of these. Natural selection alone has always seemed to me utterly inadequate to explain many of the phenomena which come under the notice of every naturalist. In fact, natural selection itself appears to require an antecedent cause. Look, for example, at the phenomenon known as protective resemblance or mimicry. If the likeness of an insect to a leaf, or a twig, or another insect, often procures its escape from its enemies, then it is easy to see how natural selection may operate in maintaining and perfecting that likeness, for those in which it is least exact will be soonest discovered and killed. But it is obvious that the resemblance must be initiated and carried a certain length before natural selection can begin to operate at all; for until the likeness of an insect to some other object is sufficient to cause it sometimes to be actually mistaken for that object, no step in the direction of that likeness can be of

any advantage to the insect. Natural selection in this case must follow the same course as human selection, which first put Darwin on the track of it. A breeder does not create varieties of pigeons, or fowls, or dogs: the utmost he can do is to seize upon any natural tendency to vary in a certain way, and perpetuate and accumulate it. Fowls often have a small crest—that is, the feathers on the top of the head are elongated; by breeding from these for many generations, and carefully selecting the most pronounced examples, the breeder produces the magnificent crest of the houdan. He cannot do the same thing with pigeons, because they have no tendency to vary in that way; but they often have the feathers on the back of the head twisted upwards, and this has been made a normal feature in several distinct breeds.

What then initiates the likeness which natural selection perfects into mimicry? Of course many causes may combine, of which some may be merely accidental, or may result from the conditions of the creature's life. The first element of likeness in an insect which mimics a green leaf is greenness, and this may be merely the result of its feeding on leaves and absorbing the colouring matter from them. A very large proportion of the larvæ of moths and butterflies are green from no other cause. The direct action of light may be another agency. Again, in view of the enormous number and variety of forms that have come upon the stage since the first appearance of life on our planet, we must allow that the doctrine of chances alone is sufficient to account for a great many superficial, or even close, resemblances. The perfectly ludicrous likeness of the pupæ of butterflies of the genus *Spalgis* to the face of a baboon, or ape, can be explained in no other way. In that instance the likeness, being of no advantage to the insect itself, could not come under the operation of natural selection, but in other cases it may have been otherwise. When, however, one considers some of the more striking instances of mimicry, together with the manner in which they are associated with certain peculiarities of habit necessary to render them effective, all these explanations together fail to satisfy the mind: one is forced to the conviction that there must be some special influence at work conforming the pattern to its copy.

Take, for example, the leaf-butterfly, *Kallima*, one of the most familiar and certainly one of the most extraordinary cases of mimicry known to us. This butterfly is brilliantly coloured on the upper surface, being chiefly blue in one species, and purple and yellow in another; but the under surface is an almost absolutely perfect representation of a dead leaf. The colour is some shade of brown, not exactly the same in any two specimens; a dark, diagonal line traces the midrib, a network of finer lines represents the venation, and there are even stains and mouldy patches, and sometimes small transparencies which look like holes in the leaf. Even the shape of the wings is modified to carry out the deception, and the ends of the posterior pair

are prolonged to form the stalk of the leaf. In connection with this extraordinary combination of form and colour, let us consider the habits of the insect. Though it inhabits shady forests and shuns strong sunlight, it flies by day, its brilliant hues of course making it a conspicuous object; but its flight is exceptionally powerful, and it has little to fear on the wing. When it requires to rest, however, it does not do so, like almost all other butterflies, on the under-side of a leaf, but on the bare trunk, or one of the larger boughs, of a tree. Another fact to be noted is that it does not feed on the nectar of flowers, but is greedily fond of the juices that exude from the wounded bark of certain trees. These habits, of course, expose it in a special degree to those tree-lizards which are by far the worst of all the enemies that butterflies have in tropical forests. The ease with which a butterfly may often be caught with the finger and thumb proves how little their imperfect eyesight secures them against an enemy approaching cautiously from behind. They see a little better in front of them, and I have noticed that the leaf-butterfly always alights head downwards, so as to face anything coming up the tree, which is much the most likely direction of assault from a lizard. [In pictures generally, and in the showcase at the British Museum (Natural History), the butterfly is turned the opposite way, facing upwards, which is, no doubt, more appropriate to its character as a leaf; but that is a detail rather above the intelligence of a lizard: at any rate, I never saw a *Kallima* sit in that position.] It also abstains entirely from opening its wings and displaying its glories to the sun, as almost all brightly coloured butterflies are so fond of doing: it sits rigidly motionless. But all these precautions would not save it without its wonderful disguise. How effectual that may be was vividly brought home to me once by another deceiver of the same character, a kind of slow cricket which exactly resembles a small patch of grey lichen. I was sitting high up in a tree, rifle in hand, waiting for a tiger, when my attention was caught by one of these crickets scurrying round the trunk of a neighbouring tree, with a lizard in full pursuit. Just as the lizard came up with it, the cricket, falling in with a slight depression in the bark, stopped dead and flattened itself out, and the lizard was utterly confounded. There it stood, looking ludicrously puzzled at the mysterious disappearance of its prey, which was just under its nose.

In both these examples we find a most extraordinary disguise associated with peculiar habits not shared by undisguised members of the same family, and these are not merely passive habits, or habitual attitudes, but active habits. That the tendency to act so under such circumstances was evolved by natural selection side by side with the disguise, even granting that there existed an initial likeness from some other cause, is not easily conceivable, at least to my mind. Moreover, there is in the behaviour of the insect something that distinctly indicates the play of at least a dim kind of intelligence. The cricket at first yielded to a primary and almost universal instinct and ran

from the lizard, but when that way of escape was clearly closed it *acted* its disguise. If we could find any reason to believe that the consciousness, or volitions, of an animal may be among the influences which have worked towards the evolution of its colour and form, then that is the direction in which I should look with most hope for the wanting explanation of these phenomena of mimicry and some others. And though, as Mr. Beddard well says in his book on Animal Coloration: "At every step in animal coloration we are met with closed doors which can only be unlocked by keys furnished by an intimate chemical and physiological knowledge such as we do not at present possess," there are not wanting facts that appear to point in the direction I have indicated. Darwin's explanation of the phenomenon of blushing is founded on the fact that "attention, or consciousness, concentrated on almost any part of the body produces some direct physical effect on it." From this he goes on to argue that "whenever we believe that others are censuring, or even considering, our personal appearance, our attention is vividly directed to the outer and visible parts of our bodies, and of all such parts we are most sensitive about our faces, as no doubt has been the case during many past generations. Through force of association the same effects will tend to follow whenever we think that others are considering, or censuring, our actions or character." "By frequent reiteration during numberless generations the process will have become so habitual, in association with the belief that others are thinking of us, that even a suspicion of their depreciation will suffice to relax the capillaries, without any conscious thought about our faces." We have Darwin on our side, then, if we believe that some effect may be produced on the skin of an animal's body, not only by its attention being directed to its own appearance, but by anything which has become associated in its consciousness with its own appearance.

Now let us turn to the chameleon. I once kept a reptile of that genus in a birdcage for six months, and grew pretty intimate with it. Its ordinary colour in the daytime, if there were no green leaves in the cage, was a dark smoky brown, sometimes with numerous, small, whitish spots; but at times, when the sun shone on it, it would indulge in a display of rich green, beautifully dappled with large, irregular marks of a lighter shade. If I approached it at such a time, it would fix one of its grotesque, rotating eyes on me, and, remaining perfectly motionless, change in a few seconds to a tint in harmony with the objects surrounding it. Absolutely ignorant as we may be of the way in which this change was brought about, two things are quite clear. The first is that the change resulted from the chameleon's becoming conscious that it was observed. It may have been quite involuntary, in which case Darwin's explanation of blushing fits exactly. Conspicuousness in its appearance being associated in the chameleon's mind with danger, the presence of danger affected its whole skin just as if its attention had been concentrated on its own

appearance. The second fact goes beyond the phenomenon of blushing altogether, and introduces an entirely new element. It is this, that the kind of change effected in the chameleon's skin was decided by the colour of the objects immediately surrounding it at that time. There appears to be little room for doubt that this effect is produced through the eyes. As to the way in which it is produced we are yet without a satisfactory explanation, but there the fact is. The physical effect produced on the chameleon's skin by the consciousness of danger is modified in some way, mysterious to us, by the colour that happens to be before its eyes at that time, so that its skin assumes that colour. It is worth noting too that its attention was concentrated on me, the source of danger; and the colour of my coat, which might be bright red, was probably at that moment irritating its optic nerve more than any other; but this had no influence. The inference seems to be that a sense of the colours of surrounding objects is blended with the consciousness of conspicuousness which affects the skin, and operates through it. The conspicuousness results of course from the contrast between its own colour and that of the things about it, and it may be this contrast which is present to the consciousness of the chameleon. A curious point that ought not to be lost sight of is that the chameleon's eyes work quite independently, so that, while one was fixed on me, the other was directed on the objects surrounding it. To what extent its mind is capable of dividing its attention we have no means of knowing.

The chameleon is not the only animal which changes colour to suit its surroundings. Many tree-frogs do so, but they change more slowly, and I cannot say from personal observation whether consciousness of danger ever has any effect in causing the change. The pupæ of many butterflies also, especially those of the genus *Papilio*, take the colour of the surface to which they are attached. They are green if suspended among foliage, but brown if attached to the trunk of a tree; and in artificial circumstances they can assume other tints within certain limits. It is not certain, however, that this effect is produced through the eyes of the larva. It may be a result of the direct action of light on the moist and sensitive surface immediately after the larva has shed its skin. It matters little, for a single case like the chameleon is quite sufficient for my argument.

Now let us try to enter into the consciousness of a leaf-butterfly sitting motionless among withered leaves, or, if that is too difficult, of a quail feeding in a stubble field. A quail's one resource in danger is concealment, to hide itself and be still. This instinct, strong at first by inheritance, has been fostered and strengthened in it by its whole life from the time when it first ran under its mother for safety. But it ventures out into the field to feed. A hawk appears in sight. If there is a bush or a tuft of grass at hand it will rush into it; failing that, it squats and remains motionless. Its state of mind as

the enemy sails overhead may, I think, be assumed to be precisely that of the chameleon with its eye fixed on me. Can it be doubted that some physical influence is being exerted by its consciousness on its skin, and if on the skin, then on the feathers growing out of it? It may be objected that the feathers are mere dead matter, beyond the reach of any vital influence, but that is not true of them while they are growing. That it is true after they are matured suggests a reason why any effect produced on the quail by its own consciousness should be permanent, and not merely temporary, as in the chameleon. Bird fanciers redden the colour of canaries by feeding them with cayenne pepper while their feathers are growing. When the feathers are mature the effect will become permanent, but further treatment of the same kind will cease to produce any result. So there is nothing in the present state of our knowledge to forbid the idea that the colour of a quail may be permanently affected by the condition of mind in which it passes so much of its time when young, viz., lying close and wishing not to be seen. And is there anything to forbid us inferring from the case of the chameleon that the change may consist in assimilation to the colours of surrounding objects, which in the case of the quail are pretty much the same always? The change may be so slight as to be quite imperceptible, but "by frequent reiteration during numberless generations" (the words are Darwin's) it may bring about that characteristic disposition of browns and yellows which makes most ground birds so difficult to see upon the ground. And it may be one of the principal influences that have been operative in bringing about many protective resemblances and mimicries. I referred to the tendency of Darwinism to lead its devotees into a very materialistic way of regarding animals. It will be difficult for those who have succumbed to that influence to allow so much significance to the consciousness of a mere insect, or even a bird, as my suggestions imply; but is it not possible that we have been all along underrating the degree of intelligence exercised by even the lowest animals in the direction of their lives, and so turning away our attention from a factor which is certainly there and may have had an unsuspected share in the evolution of animal forms? There are two faculties which broadly distinguish animal from vegetable life, namely, perception and action consequent on perception, in other words, the exercise of *mind*; and it seems to me that it is to these that we should first look for an explanation of any phenomenon which, like mimicry, prevails widely in the animal, but scarcely, if at all, in the vegetable kingdom.

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II.

The Preparation and Mounting of Chalk Fossils.

WHILE, doubtless, every collector looks upon one particular formation as the one of supreme interest, it will readily be conceded that the English Chalk occupies a warm corner in the heart of most palæontologists. Nor is the reason far to seek; for the fauna of the Chalk is so rich and so varied, the beauty of the fossils so great, the state of preservation so perfect, and the matrix lends itself so readily to skilful manipulation. Indeed, if one excepts certain intractable beds, such as the Melbourn Rock and the Chalk Rock, it is the collector's own fault if he fail to obtain an artistic result, and that with no better implements than a knife and a tooth-brush. But, unfortunately, slovenly specimens are the rule, and skilful manipulation the exception, and most private collections consist of partially cleaned specimens in shabby cardboard boxes, with loose labels, some of which may providentially be in the right tray. Even in the Government Museums one sees such imperfect work in developing and preserving fossils that one need make no excuse for touching upon apparently elementary points.

In the first place, with the exception of certain beds to be noted below, all Chalk fossils that come from sea-shore sections must be soaked in fresh water for periods varying with the size of the specimen. Having roughly trimmed the blocks, it will be found sufficient, as a general rule, to soak them for fourteen days, changing the water every day. If this be not done, the salt effloresces on the surface of the specimen as soon as the block dries, and not only disfigures the fossil but often destroys it. The softer the chalk is, the more necessary is it to remove all the salt. And in this, as in all other simple processes, attention to trivial detail is necessary, in order that a satisfactory result may be obtained. Chalk is very porous, and holds a large amount of water. For instance, a pound of dry Margate chalk will take up four ounces of water by weight, and this fact carries with it the conclusion that it is best to immerse the blocks while they still retain some of the moisture of the cliff. The reason for this is made obvious if one drops a piece of dry chalk into water: the imprisoned air is displaced by the water, and seeks the surface with a hissing noise from the liberation of the air bubbles. Should there be any crack in the chalk the sudden expulsion of the air will cause the

block to split. The best way, then, is either to moisten the blocks before immersion, or to let them stand in a shallow layer of water before filling up the bowl. Blocks which are cracked, or in which there are obvious marly veins traversing the chalk, should be wrapped in muslin lightly tied round with string before being immersed, and very damaged or precious specimens had better be moistened by means of a spray-producer before they receive their bath. Another good plan is to build a strengthening wall of plaster of Paris around the broken parts of a block. If one collects Bryozoa, and has some five hundred specimens as the result of a tour, it will be found that a daily changing of water becomes a weariness to the flesh. This may be readily and cheaply obviated by making a platform by means of half-inch wire-netting strained over a stout wire ring; and the apparatus is made more perfect by the addition of a couple of stout wire handles and four short wire supports to act as feet. The platform is made of such a size that it closely fits the sides of a large pan, near to the bottom, on which the feet alone rest. All debris then falls clear of the blocks, and settles at the bottom of the vessel. In this way fifty small blocks can be changed in as many seconds. These suggestions for soaking the blocks apply to all beds in the Upper and Middle Chalk. But with the Grey Chalk and Chalk-marl it is different, as, in the first place, the salt does not show up so plainly when the specimen is dry, and in the second, the combination of clay and lime does not readily lend itself to permeation by water. The Chalk-marl is the worse of the two, and with the exception of the large echinoderms, it is better to avoid the process, as in many instances the matrix melts away in the water. And the same, in varying degree, applies to the Grey Chalk, though the *Belemnitella plena* marl, and certain bands in the zone of *Ammonites rhotomagensis* stand the immersion well. Specimens of *Salenia* and *Pseudodiadema*, if spineless, can readily be taken out of the matrix, and they then offer a good resistance to the water.

In passing, one may mention that Gault fossils are best worked with a knife when they are slightly moist, and that beautiful results may be obtained by using a badger-hair brush. Fossils from the Thanet Sands, if tide-washed, are very difficult to preserve, as the salt simply splits them to pieces. If the block of friable sandstone is trimmed, and the edges and base well dabbed with a strong solution of silicate of potash, one can sometimes successfully soak them; but to do this one must spray them first, and then lower them into shallow water on a glass plate fitted with wire handles. These Tertiary sands are so friable that it is often impossible to carry home a specimen of any size, and in such cases it is a good plan to trench out the specimen with a knife, coat the sides of the square or oblong with silicate, *in situ*, come back in an hour's time, and remove it from the cliff.

Should Chalk fossils be worked with a wet or with a dry brush? Clearly, with the exception of the large echinoids and the more

robust brachiopods, there is nothing to be gained by the use of water, as both matrix and fossil are much more fragile when wet. An indispensable article in every workshop is an irrigator, and the thing is both cheap and simple. All that is wanted is a pail with a short half-inch leaden pipe soldered into it near the bottom. To this is attached a rubber tube, armed with a glass irrigator-point, and controlled by a spring clip. Any surgical instrument-maker will supply the two last for a few pence. If the pail is placed on a shelf, three or four feet above the level of the bench, a good stream is secured. By means of this simple contrivance one can use a fine and powerful jet of water, which will effectually wash away all the fine dust left behind by the dry brush. This plan is vastly better than treating the fossil with dilute hydrochloric acid, the use of which should be limited to the Flamborough Head sponges, the oxide of iron sponges of the Upper and Middle Chalk, and the bulky lithistids and hexactinellids of the Chalk-marl. Even in the case of these it should be employed with great caution, and then only in a very weak form.

Now, a few words as to the choice of knives. The ordinary dissecting scalpel is the best type of knife for most of the work, and in the field there is nothing better than the Swedish knife. But anyone who has cut up much chalk will have a lively recollection of the soreness of the middle joint of the index finger, due to the pressure by the back of the knife. This led me to devise the round-ended knives with a cutting surface extending from the front edge to half an inch down the back. The ease and rapidity with which one can with such a knife work off masses of chalk has to be seen to be believed, for by pushing the broad, round, cutting point one obtains all the power of the action of a chisel, with all the delicacy of finger manipulation. Further, one can be sure of not puncturing or scratching the fossil as one does with a sharp pointed knife, which as often as not breaks off as soon as any pressure is employed. One of the chief uses of these round-ended knives is to be found in working out the Plocoscyphiæ of the white Chalk, for the matrix can be scooped out of the chambers without damage to knife or specimen.

That beautiful work can be done with an ordinary knife and tooth-brush a visit to the Natural History Museum or School of Mines will abundantly prove. Even in private collections one sometimes sees beautiful results, and in none is this better shown than in the cabinets of Mr. James Fox, of Stamford Hill. But excellent as are these results, one cannot but deplore the great amount of time expended, especially when far more accurate and complicated work can be done in a tenth part of the time. It is the American dental-engine which has worked this revolution.

The engine has been employed by me for more than three years, and as fresh ideas have been evolved, so has the degree of mechanical perfection in the work increased. The use of the dental-engine was suggested while working at a fine *Cidaris sceptrifera*, for it was easy

enough to clear with a tooth-brush the top of the test and the first row of spines; but, after that, no real headway could be made, as the knife could only dig the chalk off the test, and no brush could work between the lower rows of spines. The dental-engine, with its revolving brush no more than $\frac{4}{20}$ inch in diameter, soon solved the difficulty, and cleaned the specimen so thoroughly that the little spatulate spines were left *in situ*, and not a fragment was broken off.

It may not be out of place to give the measurement of the brushes which have proved to be most generally useful. The brushes are made of hard and soft bristle, and of badger-hair, and they can be made of any length and thickness so as to suit the taste of the worker.

Small bristle brushes	i.	length $\frac{7}{20}$ in.	diameter $\frac{4}{20}$ in.
	ii.	,, $\frac{5}{20}$,,	,, $\frac{3}{20}$,,
	iii.	,, $\frac{4}{20}$,,	,, $\frac{2}{20}$,,
Small badger-hair brushes ...		,, $\frac{7}{20}$,,	,, $\frac{3}{20}$,,
Large and hard bristle brushes		,, $\frac{9}{20}$,,	,, $\frac{4}{20}$,,
Large badger-hair brushes ...		,, $\frac{10}{20}$,,	,, $\frac{8}{20}$,,

With brushes like these one can do most things. *Cidaris sceptrifera* was cited as an example of what the engine can do, but this is coarse work compared to much that can be achieved by this instrument. The delicate *Plocoscyphia convoluta* of the soft Margate Chalk can be cleaned out so that chamber opens into chamber, and a bristle may be passed from one cavity to another, the hexactinellid mesh being left intact, and the walls of the convolutions standing out unbroken, though no thicker than a stout visiting card. This sponge, to the best of my knowledge, has never been worked out before, and, but for the round-pointed knife and the dental-engine, it would probably still be undeveloped.

But even more striking than this is the result obtained with the Bryozoa, especially with the delicate branching Cyclostomata. Here, instead of being content with tediously hand-brushing a fallen fragment, one can take a colony, with its intertwining branches, and clear all the matrix away, leaving the beautiful structure supported by only a few points on the block, with every cell sharp and clear. In my collection there is a colony of *Pustulipora pustulosa*, two inches square, with branches under $\frac{2}{20}$ of an inch in diameter, cleared in this way, and so delicate is the work of the engine that not a particle of the branches was broken off in the process. *Reticulipora obliqua* can be taken out of the matrix, and all the mesh-work cleared out, so that not an atom of chalk remains. Such delicate Bryozoa as *Entalophora proboscidea*, *Vincularia regularis*, *Heteropora francquana*, and the like can be displayed on the matrix with a badger-hair brush, without the least damage to the specimen.

The student of Foraminifera, Entomostraca, and Bryozoa, who has the good fortune to work in a district where spongiform flints abound, has his task made easy, for the flint-meal will yield him

countless treasures, which only require to be washed in water to be ready for mounting. It is very different for those who work in districts where the flint is compact, for there every specimen must be laboriously worked out of the chalky matrix.

Every collector of Bryozoa must have been struck with the number of beautiful forms to be found in a cliff section, which are useless, by reason of the weathering of the exposed surface. The dental-engine offers a very simple solution of this difficulty. The process is as follows. Thoroughly clean the exposed surface with the badger-hair brush, and work it so finely that the specimen is nearly undercut. With a sharp pointed knife free the specimen from the block, taking away the smallest possible amount of matrix with it. Cement it, worn surface downwards, on some suitable rough surface, and when dry work the unweathered surface with the badger-hair brush. It is important to apply the gum only to the fossil itself, and to leave the matrix untouched by the cement. Soak off the specimen in hot water, and mount on a slip of black cardboard. An excellent thing to fix the specimen upon is a glass slip that has been roughened in the centre with hydrofluoric acid. Kay's cement has proved very useful in my hands. Bryozoa as thin and fragile as *Entalophora proboscidea* and *Vincularia regularis* can be treated in this fashion. In a like manner all small fossils, such as macroscopic Foraminifera, little brachiopods, and small Serpulæ can be removed from their matrix, and mounted in any position on black cardboard.

These instances are quoted as examples of the delicate work which one can get out of the engine; but it must not be supposed that practice and deft fingers have nothing to do with the process. Those who are engaged with the zonal distribution of the Bryozoa will find the engine most helpful in obtaining a rapid determination for making a list. If one has to handle several hundred Bryozoa from one zone, it is manifestly impossible to clean them all perfectly, and a few touches of the brush will put the determination beyond doubt, and at the same time tell one if the specimen is worth working. Bryozoa like *Truncatula*, *Semicytis*, *Homæosolen*, and *Idmonea* require to be worked on both sides to establish a determination, and in these cases the rapid work of the engine is simply invaluable.

But the dental-engine can do hard work equally well, and perhaps no better chance of testing its powers could be found than the effectual cleaning of large masses of Chalk-marl sponges, such as *Plocoscyphia labrosa*, *Craticularia fittoni*, and all the host of intractable lithistids. In this case the bristle-brush is replaced by the cutting rose-headed bur, and the steel and brass brush, and it is astonishing to see the way in which the hard matrix melts away before the business-like little tools, and the manner in which the sponge structure is revealed, but not destroyed. In fact, so much can be got out of the engine, both in power and delicacy, that the results leave little to be desired. Given a reasonably workable chalk, the specimen should

be so thoroughly cleared by the brush that it should successfully stand the scrutiny of a strong hand lens. Indeed, no specimen can be considered clean until it will pass this ordeal.

The bristle brush can be used for working fossils in a damp state, and if the Grey Chalk and Chalk-marl specimens are worked in a moist condition (not soaking wet) beautiful results may be obtained, even in such awkward cases as clearing the long spines of *Ostrea carinata*. The Grey Chalk Salenias and Pseudodiademas are often quite impossible to clean in a dry state, and if care be taken to avoid using too much water, the urchins may be freely handled.

Anyone who has attempted to clean echinoderms of the nodular chalk of Dover, of the Melbourn Rock, or of the Chalk-marl, will have been reduced to despair by the way in which portions of the matrix decline to be removed. A very good plan is to reserve the stumps of the half-worn bristle brushes for these obdurate specimens, or in the case of the Holasters and Hemiasters of the Grey Chalk and Chalk-marl, to use the fine brass brush. This brush is of the same size as No. i. bristle brush, and is made of crinkled brass wire, as fine as silk, and it does no harm. Some patches, however, are so grown into the surface of the shell that nothing will remove them, and the dry brush will even polish them.

If one may judge from the grimy state of many Chalk fossils in public museums, not one of the least useful functions of the engine will be found in the case of the large badger-hair brush, which takes a thin film of matrix off flat surfaces without cutting into the specimen. Thus a fresh surface can in a few seconds be given to a block, without laboriously paring away the chalk with a knife, with the certainty of reducing the specimen to the vanishing point in course of time.

While on the subject of touching up old specimens, it may be well to mention that all hard fossils can be safely cleaned by dabbing them with cotton wool soaked in methylated ether. Many specimens would split if water were brought in contact with them, but the ether evaporates so rapidly that no harm is done.

The variety of instruments which can be fitted into the hand-piece of the engine varies only with the ingenuity of the worker, and special burs can be used for smoothing down the surface of Chalk-marl blocks, or drills can be made for piercing them.

As a lubricant for the engine nothing excels a white odourless hydrocarbon oil, which goes under the name of oleum deelinæ, or paroleine. Moreover, one can keep the steel burs, brushes, and drills in the same excellent fluid; and if a wide-mouthed bottle is filled with river-sand soaked in the oil, the scalpels can be stuck into the sand, and left with impunity even in a damp workshop.

The choice of an engine is a matter of no small moment, both from the point of view of cost and efficiency. What is wanted is a short, second-hand instrument, which is easy to work sitting down. Never mind if the engine is not a thing of beauty, so long as it goes

well, and do not be induced to buy one of those tall silver-plated erections which cost from £10 upwards. What is known in the trade as a "S.S.White Engine," with flexible coil and short stem has served me for three years without breaking down, and one wants nothing better than that. If the machine does not work easily while one is sitting down, discard it at once, as a proper instrument will enable one to work for hours without causing fatigue. Indeed, when one gets used to the movement, it becomes almost a reflex action. Above all things, in choosing the engine, fit a brush or bur into the hand-piece, and see if it travels true. If the hand-piece 'wobbles,' the brush will deviate from the straight line and describe a small circle, and it will inevitably smash every specimen with which it comes in contact. This engine should not be a costly affair, and should be within the reach of most large collectors; indeed, I venture to say that no workshop will be complete without it. An excellent engine can be bought for £2 10s., and the small bristle brushes cost a penny each, and the steel burs about 10s. a dozen. The latter can be recut for a trifling cost when they are blunted. The softer bristle brushes, and those made of badger-hair, should be made at home, so that they can be made to suit the specimen in hand. The hand-piece of the engine is the only part likely to get out of order, and occasionally that gets clogged with dust, and has to be detached and sent to the dental depôt.

It is impossible, in a short communication like this, to describe the actual working of the various little tools, or to describe the tools themselves, or the manufacture of the brushes. The makers will be pleased to show them to anyone who is interested, and it will be a pleasure to me to give a short demonstration on the process to those who care to call upon me. The knives and steel brushes can be obtained only of Down Brothers, St. Thomas' Street, Borough, and the large bristle brushes only from Claudius Ash & Sons, Broad Street, Golden Square. The burs and small bristle brushes are supplied by Rutterford & Son, 11 Poland Street, Oxford Street. A dental-engine can be got from Rutterford or Ash, and it is worth while to get a friendly dentist to help you to chose one.

No claim is made for any special originality in this application of the dental-engine, but it is asked that the method should have a fair trial, as it is reasonable to think that it is destined to revolutionise the cleaning of fossils in soft rocks, and to enable the palæontologist to develop specimens which have hitherto been severely left alone. Nobody who has once experienced the satisfaction of working with this simple mechanical assistance will ever revert to the tedious and clumsy method of knife and tooth-brush, and the mere time saved will alone repay the worker for the small outlay. This method of cleaning fossils has recently been introduced into the Natural History Museum, Cromwell Road, and into the Woodwardian Museum, Cambridge.

A few suggestions as to mounting fossils when they are cleaned

might not be out of place, although, no doubt, every collector has his own method.

Cardboard trays and loose labels are an abomination: delicate specimens can be put in boxes with glass tops, and, in any case, every specimen should have its own label fixed to it in some way. My plan is to mount each specimen or series of specimens on a wooden tablet, upon which is written the name, zone, exact locality, and a reference to some accessible figure of the species. The tablets may be in multiples of each other. The best wood for making them is that of the American poplar, known as American white-wood. This is soft enough to allow thin pins to be driven in without bending, and firm enough to prevent a heavy specimen from pushing the pins out of place. The wood is covered with paper such as "Hieratica" or "Silurian," with the aid of a spirit-gum known as "parlour paste" and a copying-press. The edges of the paper should be trimmed off neatly with a round-pointed knife. The specimens are attached to the mount by pins: headless pins of any length or thickness may be obtained from D. F. Tayler & Co., New Hall Works, George Street Parade, Birmingham. Specimens that have to be examined with a lens, such as Bryozoa, Foraminifera, and Entomostraca, should be kept under glass, since they will rarely stand a second cleaning. Many collectors of Bryozoa mount their specimens on wooden microscope slides with a circular aperture backed by black paper. The plan has its advantages, but the specimens get dirty, and their size is necessarily limited. I mount Bryozoa on black cardboard slips, and enclose them in a glass tube which is fixed with pins to a wooden mount on which the label is written. The tubes that I use all have a length of $1\frac{1}{4}$ ins. (about 31 mm.), with a diameter of $\frac{1}{8}$ in. (3·5 mm.), $\frac{1}{2}$ in. (12·5 mm.), and $\frac{3}{4}$ in. (19 mm.) Tubes of this or other sizes may be bought by the gross from Maw, Son & Thompson. If the surface of the cardboard used is too greasy to take the gum, this can be remedied by rubbing it with methylated ether.

A cement is necessary for fixing small specimens to the tablets, and for mounting broken ones. Fish-glue sometimes sinks into the chalk, and, if the museum is at all damp, often causes mould to form and the specimen to be detached. Gum and plaster of Paris is an excellent cement, clean and strong, but the desideratum is something which will set instantly, and will enable the specimen to be as readily detached in case of need. Such a material is found in "dental model cement," which is probably a compound of shellac and white wax, and is procurable at any dental depôt at a cost of twopence a stick. A spirit lamp is the only accessory. The value of the cement lies in the fact that it is white and clean, always ready, and reasonably strong; and that anything as thin as a sixpenny-piece can be stuck up on end on the mount, and very thin surfaces, such as the fragments of an echinoderm, can be instantly fixed together. Moreover, large masses of chalk may be at once cemented together, and the cement

has this advantage, that the chalk can be afterwards treated with water or hydrochloric acid without in any way impairing its strength. The cement is soluble in benzol, and any bead of it which exudes between joined surfaces can be rubbed away with cottonwool soaked in that fluid, or flaked off with the point of a knife, or pared away with a blade which has been heated in the spirit lamp. To detach a specimen from its mount, it is only necessary to heat it, or to pass under it an old flexible table-knife which has been heated. When the chalk is soft, it is better to soak the bottom of the block with silicate of potash, as otherwise the cement may adhere to the mount, and drag away with it a layer of the chalk. A very useful aid to heating awkward corners and angles of a specimen is to be found in the spirit lighter, called a promethean, which is used to light incandescent gas burners.

Let me conclude by a plea for the more exact localisation of fossils. For this one usually looks in vain, and "South of England" seems to be considered near enough for all practical purposes. As to zonal localisation, there is so little evidence of such a custom in high places that it would be wonderful if it were commonly found in private collections. But if a collector cannot name a fossil, or determine the zone from which it comes, he can at least fix the exact locality; and this it is his clear duty to do, since many notable fossils lose half their value from indiscriminate collecting. On this head Mr. A. J. Jukes-Browne, in his paper on "The Subdivision of the Chalk," gives good advice. He says: "Much, however, still remains to be done in completing our knowledge of faunas of the different zones, and everyone who makes a collection of fossils from any part of the Chalk, and is careful to record the exact zone and quarry where each specimen was obtained, will be rendering valuable assistance."

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III.

Zoology since Darwin.[1]

Part II.

ZOOLOGY profited much by the countless researches which consciously or unconsciously led towards the solution of the problem of heredity and variation.

Darwin accepted heredity as a fact proved by the specific resemblance between parent and child, without seeking after its material carriers.[2] Since all living beings consist of cells, and even the most complicated organism has in the first beginnings of its development the form of a simple cell, these carriers could only be portions of cells. To find them was left to a deeper insight into the build of the cell and into the meaning of its parts. As the structure of the cell-substance and the nucleus contained therein was investigated with new methods, the surprising fact was discovered that movement and irritability as well as respiration were functions exercised by the cell-substance independently of the nucleus, and that on the other hand assimilation and secretion followed only under the influence of the nucleus, and that this was the only organising form-building factor of the cell. Further research showed the nucleus to consist of two substances, chromatin and achromatin, easily distinguished by their re-action to colouring agents; the first of these plays an important rôle in cell-division, since its changes introduce the process of multiplication, and lead, through regularly successive typical forms (mitosis), to division first of the nucleus, and then of the extra-nuclear cell-substance.

The conclusion that chromatin was the true heredity-substance could only be deduced with convincing clearness after duo-parental (amphigonic) reproduction had been studied. Here, where two cells, usually of different form, combine to form an egg capable of develop-

[1] Lecture delivered by Professor Ludwig von Graff on his installation as Rector Magnificus of the K. K. Karl-Franzens University in Graz, November 4, 1895. The profits from the sale of the original go to the Freitisch-Stiftung of the University. (*Continued from p.* 198.)

[2] Darwin's Pangenesis hypothesis, which assumed the germ to be influenced by the body in which it was buried, as well as by the prevailing external conditions, in such a way that minute particles were continually added to it from all parts of the body, has no actual foundation, and is, as Weismann says in his "Keimplasma," p. 7, "more a statement, than an explanation, of the problem of heredity."

ment, the conclusion was confirmed that the chromatin of the egg arose in equal parts from the chromatin of both parent-cells.

Thus was solved the problem of centuries—the problem of fertilisation, whose story shows, in the most instructive and, at the same time, most amusing manner, how preconceived ideas can dim observation, and how ingenious the human mind becomes when it is a question of supplying want of fact by dialectics.

Next to phylogenetic research, investigations on cell-division and fertilisation stamp the post-Darwinian period of zoology. They are among the most brilliant achievements in the domain of natural science. The zoologists who took chief part in them, W. Flemming, O. and R. Hertwig, Ed. van Beneden, were the first[1] to render possible the exposition of a theory of heredity. For, weighty as may be the most modern discovery that all movements of chromatin are accomplished passively, guided and directed by a source of energy placed in the newly-found centrosome, it effected no change in the interpretation of the chromatin as the substance that transmits the parental characters.

Since all cell-nuclei come from the nucleus of the ovum, there is in all body-cells a part of the parental chromatin, and thereby an inheritance of the parental characteristics is insured. On this foundation rests Weismann's theory of heredity,[2] to which we cannot deny the one merit of having at last clearly formulated the question of inheritance.

Are the ways of evolution exactly determined beforehand by the constitution of the germ, or is the germ a mass of formative substance, indifferent up to a certain point, and governed in its further development solely by the conditions to which it is exposed?

The descent of modern organisms from ancestors of different form on the one hand, and the facts of heredity, which teach us that parent and child (or, broadly put, the successive generations of the same species) always pass through specifically similar stages of form on the other hand, give the answer to this question. As the orthodox Darwinian expresses it: Every organism is the result of heredity and adaptation; what the parent inherits it transmits in its entirety to the child, but adds thereto what it has itself acquired.

Weismann's Neo-Darwinism, in opposition to this, denies the inheritance of acquired characters; and there exists neither a single undoubted fact to disprove his teaching, nor any theory that can

[1] *Note added by Author, October*, 1896: When writing this part of my address, I was fully aware that the name of O. Bütschli should have figured in *the front rank* of researchers on cell-division and fertilisation. Its omission was due to a *lapsus calami* which I deeply regret, but which I cannot understand; indeed, I only became aware of it through reading Bütschli's review of my address in *Zool. Centralblatt*, iii., p. 421. I am glad that this publication in NATURAL SCIENCE enables me in a measure to repair the omission.

[2] A. Weismann, "Das Keimplasma: Eine Theorie der Vererbung." Jena, 1892. This author has also dealt with the question in "Die Allmacht der Naturzüchtung: Eine Erwiderung an Herbert Spencer." Jena, 1893. "Aussere Einflüsse als Entwickelungsreize." Jena, 1894. "Neue Gedanken zur Vererbungsfrage." Jena, 1895.

render plausible the idea that new characters appearing in the organs of the body are transferred to the germ-cells. If we accepted this, or —what comes to the same thing—a direct action of the environment producing heritable modifications, we should have to admit that from the eggs of any single species new forms of the most varied description, capable of life and reproduction, could be obtained by simply changing the conditions of existence. This—which is contrary to all experience—is the final consequence from the assumption that external conditions are the factors which decide form. O. Hertwig and Y. Delage[1] seek to escape these conclusions—which they too consider absurd—by attributing the essential similarity of child and parent to the specific chemico-physical composition of the germ-plasm derived from the parent. The production of one species from the germ of another would always be prevented, because every germ must die which does not find the life-conditions adequate to its specific composition. But this means nothing else than that the causes of form lie in the internal composition of the germ.

In the face of these true causes of form inherent in the germ itself, to attribute equal causal influence to external conditions is obviously arbitrary, and, moreover, implies—as F. von Wagner[2] has shown—a confusion of "cause" and "condition" contrary to the customary use of the ideas.

The System and the comparative anatomy of organisms show us that a seemingly infinite number of developmental possibilities lie open to the germs, and therefore if certain germs always pursue the same path as did their countless generations of ancestors, this can be due only to internal causes; the path, however, in order to be followed, must lie open—that is, there must exist certain external circumstances ("conditions"), in order that the causes may result in visible action.

[1] Y. Delage, "La Structure du Protoplasma et les théories sur l'hérédité et les grands problèmes de la biologie générale." Paris, 1895. Delage's views on heredity are the exact antithesis to Weismann's. Characteristic is his contention that in order to explain the morphological and physiological agreement between parent and child, the idea of a hereditary substance inherent in the germ is no more required than it is to make us understand why the corpse of a worm, of an insect, of a frog or of a mammal, under the same external conditions, always undergoes decomposition in its own peculiar way, each creature having its own typical process.

O. Hertwig, "Präformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen." Jena, 1894. English translation by P. Chalmers Mitchell, reviewed in NATURAL SCIENCE, vol. ix., p. 270. Hertwig stands midway between Delage and Weismann, inasmuch as he does not put the causes of form exclusively in the germ, or exclusively or principally in external conditions, but thinks them both equal in their influence. Hertwig says himself of his theory (pp. 132–133):— "My theory may be called *evolutionary*, because it assumes the existence of a specific and highly-organised initial plasm as the basis of the process of development. It may be called *epigenetic*, because the rudiments grow and become elaborated, from stage to stage, only in the presence of numerous external conditions and stimuli, beginning with the metabolic processes preceding the first cleavage of the egg-cell, until the final product of the development is as different from the first rudiment as adult animals and plants differ from their constituent cells."

[2] F. v. Wagner, "Some Remarks on O. Hertwig's Entwickelungs-Theorie" (*Biologisches Centralblatt*, xv. Bd., pp. 777–815, Leipzig, 1895), and "Das Problem der Vererbung" (*Aula*, i. Jahrg., nos. 24 and 25, Munich, 1895).

E. Haeckel has asserted[1] that the whole theory of descent stands or falls with the inheritance of acquired characters. This would be so only if there were no other explanation for the variability of the organism. However, even Weismann does not deny that the conditions of existence exercise an influence over the constitution of the germ-plasm, and may, during long periods, strengthen many components in the combination of forces of the latter, weaken others, and even introduce new sources of energy. But these influences will only act directly, and not through the medium of other organs of the body.

In order to accept this direct influence of the germ-plasm by external agents, we are specially directed to those organisms with monogonic reproduction. In amphigonic organisms, the simple mixture of the inheritance substance of two parent-individuals produces an extraordinary number of possible combinations in the composition of the germ-plasm—so many that the extrusion (observable before the beginning of development) of a part of the nucleus of the ovum (the so-called polar body) was believed to indicate an arrangement for the elimination of the superfluous inheritance-tendencies.[2]

The cause of variation lies then in the individually changing composition of the germ-plasm, and what we call "adaptation" is no active and direct performance of the individual, but is the result of a more or less complicated selective process working through generations of individuals. The external conditions, to whose influence animals respond in exact, regulated ways, peculiar to each species, are therefore in no wise the real causes of such reaction, but release a formative energy, latent in the germ, and brought into action only under these definite conditions.

If we accept the view here set forth, there results a noteworthy contrast between the germ-cells and the "soma"—that multiform and often extraordinarily complicated combination of the remaining organs of the body. Principally in this, that in the segmentation that takes place during the development of the egg, those cells which in the perfected organism will play the rôle of germ-cells, receive a portion of all those parts of the egg which are necessary for the re-formation of a complete individual; while, in the soma-cells, only those substances necessary to the formation of a particular organ or organ-complex are transmitted. At the same time, however, the soma becomes a kind of organ of the reproductive cells, which accomplishes the work of assimilation, moves, feels, and thinks—but thereby serves in some respects only the development of the germ-cells, and so ensures the continuity of life.

(To be continued.)

[1] E. Haeckel, "Zur Phylogenie der australischen Fauna. Systematische Einleitung." In R. Semon, "Zoologische Forschungsreisen in Australien und dem malayischen Archipel," i. Bd., Jena, 1893.

[2] A. Weismann, "Uber die Zahl der Richtungskörper und über ihre Bedeutung für die Vererbung." Jena, 1887.

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IV.

An Introduction to the Study of Anthropoid Apes.—III. The Orang-Outang.[1]

MATERIAL for the study of the orang has always been more plentiful than that for the study of the chimpanzee or gorilla. Consequently the literature on this animal is the more extensive, and founded upon a wider basis of observation. Some idea of the amount of material may be obtained from the fact that within the last fifty years about seven hundred skulls have been collected for purposes of exhibition and study in museums. Skeletons and stuffed specimens are not so plentiful as skulls, but yet common enough, especially in museums, such as those of Holland and India. Living animals, too, are not unfamiliar exhibits in Europe. In the zoological garden of Rotterdam there have been twenty specimens in the last forty years, and about an equal number in the gardens at London. In captivity they do not, as a rule, live long, five years, perhaps, being the longest, and two months being the average length of life (Schmidt, 274*a*). The supply of animals has been fairly abundant, yet descriptions of the soft parts of their anatomy are very few. Perhaps the most complete are those added quite recently by Milne Edwards (258), with the assistance of colleagues, and by Fick (235), although valuable contributions to the general anatomy of the animal had previously been made by Sandifort (271), Beddard (93), and Chapman (229). The four animals dissected by Milne Edwards and Fick were full grown and the first mature adults seen in confinement in Europe. Much that is known of the anatomy of the orang is included incidentally in descriptions of the anatomy of other animals—*see* Vrolik (210), Giglioli (31), and Bischoff (293).

The Nervous System.—As far as I am aware, the nerves of the head and trunk have never been investigated, but, on the other hand, the nerves of the extremities have been well described by Westling (287), Hoefer (140), Kollmann (150), Hepburn (45), and slightly by Fick (235, 127). The lumbar plexus has been figured by Utschneider (209), Jhering (143), and Westling (287). No description has been given of the visceral nerves. The microscopic structure and minute anatomy of the centres and tracts of the cerebro-spinal axis remain

[1] Temminck gives *Orang-Outan* as the correct spelling; Sal. Müller, who was familiar with the Malay language, rendered it *Orang-Oetan*, but *Orang-Outang* is the form in most common use.

untouched. It is quite different as regards the surface anatomy of the brain. The fissures, lobes, sulci, and convolutions of brains of individual orangs have been described and figured by Bischoff (222), Chapman (229), Fick (235, 127), Rolleston (266), Sandifort (271), Schroeder and Vrolik (275, 196), Tiedemann (281), and Barkow (90). The surface anatomy of the brain has been treated in a more general way, and from a greater number of specimens, by Cunningham (115, 116), Benham (94), Bischoff (221), Gratiolet (130*a*), Huxley (49*b*), Kükenthal and Ziehen (151), Pansch (176), and Eberstaller (298*a*). The Island of Reil and the third frontal convolution have been specially studied by Cunningham (115, 116, 117), Hervé (48), Marchand (157), and Rüdinger (188*a*). As to the relative development of the lobes to each other, the relationship of the cerebellum to the cerebrum, and of both to the cranial walls, one may consult Cunningham (118), Féré (234), Flower (237), Schroeder and Vrolik (196, 275), Hamy (136), and Rolleston (266). Beevor and Horsley (220) have investigated the motor areas of the cortex, and Hitzig (246*a*) has made some observations respecting the morphology of these areas. The weights of the several parts of the brain have been estimated by Moeller (166), and Keith (146) has dealt with the brain weight as a whole.

The Muscular System.—There is not to be found anywhere in literature one complete account of the muscular system of the orang. The fullest descriptions are those given by Beddard (92), Chapman (229), Church (231), Fick (235, 127), Huxley (49*b*), Owen (263, 261), Vrolik (210), and Sandifort (271). A great deal can be ascertained from drawings given by Cuvier (231*a*) and Barkow (90), and something from the writings of Camper (228), Mayer (160), and Westling (287). The muscles of the extremities have received most attention, having been fully described by Hepburn (45), Langer (252), and Barnard (219). Quite a number of anatomists have paid attention to certain muscles or groups of muscles: Lartschneider (152*a*) to the muscles of the pelvis; Ruge (70), Bischoff (7), and Rex (67*a*) to the muscles of the face; Bischoff (100) to the extensor indicis and flexor pollicis muscles; St. John Brooks (106, 107) to the short flexor muscles of the thumb and little finger; Keith (148) to the rectus abdominis thoracicis and flexor profundus digitorum; Ruge (190, 268, 269) to the rectus abdominis, muscles of the planta and extensors of the toes; Koerner (251) and Mayer (161) to the muscles of the larynx; Ottley (260*a*) to the muscles of the eye; Westling (287) to the muscles of the tongue and pharynx; Seydel (198, 199) to the serrati muscles of the back. Langer (252) and Fick (235) record the weights of most of the limb muscles, a labour which seems well worth the extra trouble, as weight is far the best index of degree of function. By adding together these various accounts, one may obtain a fairly complete picture of the muscular system of an orang. The involuntary muscles still require investigation.

Ligaments and Joints.—Very little attention has been given to the ligamentous or articular structures of the orang. Fick (235) gives many details concerning the articulations; Aeby (88) and Thomson (204) of the ankle joint and foot; Hartmann (39) of the hip joint, while Keith (250) gives a description of many of the ligamentous structures.

The Skull.—In the examination of a large collection of orang skulls, such as is available in London through the courtesy of the curators of the leading museums (British Museum (Natural History), Royal College of Surgeons, and Royal College of Science), including over eighty skulls, one is struck by the amount of variation they exhibit. This great diversity of form is due, for the most part, to the fact that at no time of life does the orang's skull cease from growing and changing, altering nearly as much in old age as in youth; but it is also due to the fact that there is a great amount of individual variation. It is owing, in a lesser degree, to the fact that skulls are impressed to a variable extent with sexual characters. It is commonly quite easy to tell the skull of an adult female from that of a male, but at times this is hardly possible, the skull of the female having assumed characters commonly found in the male or *vice versâ*. It is worthy of remark, however, that skulls coming from the same locality have a striking similarity of form, even to the minutest features. These observations will help to explain the vast literature that has arisen around the skull of the orang. The wide fluctuation in form and size was thought to be due to there being several species of orang, and an immense amount of labour was expended upon skulls to discover the cranial characters of each species by Blyth (224), Brühl (227), Fitzinger (236), Temminck (280), Schlegel and Müller (272), Giglioli (31), Lucae (252*a*), Wagner (285*a*), and many others. There can be no doubt that the crania in the Natural History Museum at South Kensington, assigned by Owen (262) and Wallace (284) to *Simia morio*, are the skulls of scarcely mature individuals, and all the characters assigned to them as specific are those which distinguish the skulls of young from fully adult animals. Lucas (253), on the other hand, concluded that the cranial characters of *Simia wurmbii*, Fischer, were simply marks of old age, a conclusion with which I agree. What is really wanted at present more than aught else is a thorough examination of a wide series of skulls, perhaps three hundred might be sufficient, including all ages, and a final determination of the characters due to age and sex changes and those due really to individual irregularity. Selenka (277) appears to possess ample material for such an enterprise, but as yet he has only published a very brief review of his collection. Age changes so far have been noted by Dumortier (232, 233), Heusinger (246), Mayer (257), Temminck (280), and Lucas (253), who had at his disposal the large collection brought home by Hornaday (247) from Borneo. The generic characters, which distinguish the skull of the orang from that

of the gorilla or chimpanzee, have been dealt with by a very great number of writers—Bischoff (3), Brühl (227), Duvernoy (22), Huxley (49*b*), Geoffroy St. Hilaire (72), Owen (264, 175), and Hartmann (40). Figures or descriptions of one or more skulls are given by Abel (217), Barkow (90), Bolau (10), Anderson (218), Camper (228), Fick (235), Giebel (241), Hervé (245), Mayer (160), Meyer (58), and Schlegel (193). Concerning the craniology of the orang skull Bischoff (8) and Delisle (258) may be consulted. Certain special points of structure have been studied: the nasal bones by Chudzinski (113), Maggi (255), Mayer (257); the nasal cavities by Seydel (178) and Zuckerkandl (216); the anterior nasal spine by Hamy (35); the sutures of the premaxilla by Maggi (254); the posterior palatine spine by Waldeyer (211); the lachrymo-ethmoidal suture by Regnault (182); the orbito-maxillary-frontal suture by Thomson (205); the cranial capacity by Selenka (277) and Keith (146).

The Skeleton.—There has been no investigation made of a collection of skeletons, most descriptions being of a general nature only, and drawn from a single specimen—*see* Blainville (223), Owen (264, 175), Huxley (49*b*), Mivart (61*a*, 259), Hervé (245), Duvernoy (22), Vrolik (210), and Hartmann (43). Some information of value may be picked from the accounts or figures of Trinchese (283), Temminck (280), Schlegel (193), Meyer (58), Fick (235, 127), Camper (228), Bolau (10), Blyth (224), and Barkow (90). Certain parts have been specially studied, such as the sacrum by Broca (104) and Paterson (179); the vertebral column by Cunningham (118); the bones of the hands and feet by Lucae (54), Hartmann (138), Lazarus (153), Rosenberg (187*a*), Kehrer (249), Thomson (204), and Trinchese (283); the manubrium sterni by Keith (149). Measurements of the limb bones have been given especially by Rollet (186*a*), Delisle (258), and Temminck (280).

The Teeth.—Topinard's (82) description of the molar and premolar teeth is the most complete given as yet, but the general treatises of Owen, Huxley, Tomes, and the more particular descriptions of Magitot (56, 57), Giebel (240), and Duvernoy (22) are well worthy of reference. Abel (217), Barkow (90), and Fick (127) add minor accounts. No investigation has so far been made of the development of the teeth, but recently Selenka (277) has contributed some valuable data concerning the order of eruption of the dental series. Orangs' teeth are much subject to variation. A form of macrodontia, sometimes almost pathological, is common. Irregular and supernumerary teeth are of frequent occurrence—*see* Selenka (277), Bateson (91), Brühl (227), Lucas (253), Maggi (256), and Schmidt (195).

The Alimentary System.—The literature on this system is brief and incomplete. The arrangement of papillæ and the structure of the tongue have been subjects of investigation by Fick (235, 127), Mayer (162), Deniker and Boulart (258), Sandifort (271), and Westling (287). The rugæ of the hard palate are figured by Beddard (92) and Gegen-

baur (238). Brief and general descriptions of the alimentary tract are contributed by Chapman (229), Fick (235), Flower (28), Huxley (49*b*), Mayer (160), Owen (261), Camper (228), Sandifort (271), and Barkow (90). Wittmann (288) gives very exact figures, showing the arrangement and distribution of the blood-vessels of the stomach and bowel. Figures or descriptions of the liver are given by Barkow, Chapman, Fick, Flower, Camper, and Sandifort (*opp. citt.*). Chapman also describes the arrangement of the peritoneum.

The Respiratory System.—The laryngeal sacs have frequently been subjected to examination, the result being to show that although at first there are two sacs, one from each ventricle, they may ultimately become one by the absorption of the separating wall—*see* Fick (235, 127), Deniker and Boulart (19, 258), Sandifort (271), Camper (228), Mayer (161), and Chapman (229). Descriptions of the larynx may be found in Koerner (251), Mayer (161), Sandifort (271), Camper (228), Westling (287), Cunningham (118), and Fick (235). Aeby (88*a*) and Chudzinski (230) have given descriptions of the lungs, while nearly all the writers cited in this section have made some observations upon the pulmonary apparatus. Ruge (189) and Tanja (203) have mapped out the limits of the pleural cavity.

The Circulatory System.—The only observations upon the form and size of the heart are those of Sandifort (271), Fick (235), and Chapman (228). The arteries and veins have been done in part only. Barkow (90) gives figures of the circle of Willis and of many vessels of the extremities. Popowsky (265) has described the arteries of the lower extremities; Wittmann (288) the vessels of the alimentary tract; Keith (147) the arrangement of the arterial trunks from the aortic arch; Fick (235), Westling (287), Camper (228), and Sandifort (271) record some observations on the arterial system. Gulliver (135) gives measurements of the red blood corpuscles, and Ruge (189) describes the position of the heart.

The Genito-Urinary System.—The best description of the male genital organs is given by Pousargues (258), less full accounts being those of Sandifort (271), Barkow (90), Chapman (229), Fick (127, 235), Mayer (160), Camper (228), and Crisp (114). Trinchese (283), Bischoff (6), Camper and Barkow (*opp. citt.*) describe very briefly the genitals of the female. Slight references to the urinary organs may be found in Sandifort (271), Huxley (49*b*), Chapman (229), and Barkow (90).

Psychology.—The motor centres have been localised by Beevor and Horsley (220). The habits of the orang have been studied in the jungle by Wallace (285), Müller (272), Hornaday (247, 248), St. John (270), Mohnike (260), and Wenckstern (286); in captivity by Darwin (120), Bolau (225), Deniker (121), Fick (127, 235), Delisle (231*b*), Friedel (129), Hermes (139*a*), Martin (159), Reuvens (184), Grant (242), Camper (228), Brehm (225*a*), and Sclater (276*a*).

Organs of Sense.—The circumvallate papillæ have been

examined by Mayer (162); the olfactory organ by Zuckerkandl (216), and the touch papillæ of the hands and feet by Kollmann (150).

External Characters, Configuration, and Measurements.—There are quite a large number of very fine drawings of the orang, such as those of Fick (235), Milne Edwards (258), Hermes (244), Beddard (93), Schlegel and Müller (272), and a photograph of a fœtus by Trinchese (283). Among those that have given particular attention to the characters of the hands and feet are, Abel (217), Alix (89), Barkow (90), Beddard (93), Fick (235), Harwood (243) Hepburn (46), Kollmann (150), Lucae (54), Trinchese (283), Temminck (280), and Camper (228). The curious cheek-pads that stand out as stiff flaps from the cheeks of some orangs have been specially dealt with by Fick (235), Deniker and Boulart (258), Temminck (280), Brooke (226), Selenka (277), and Möbius (259*a*). Numerous other writers give the cheek-pads a passing notice, but even yet their use, constancy, and significance are unknown. The hair, its colour and arrangement, has been described by most of the observers just cited, and to make the list more complete one must add Bolau (225), Chapman (229), Delisle (231*b*), Deniker (121), Fitzinger (236), Friedel (129), Hartmann (40, 43), Geoffroy St. Hilaire (239), Hornaday (247, 248), Meyer (58), Martin (159), Meijere (163), Reuvens (184), Schlegel (193), Wallace (284, 285), Grant (142), Möbius (259*a*), and Wenckstern (286). The peculiar arrangement of the hair is due to the fact, that when the animal lies on its side, as it does in sleep, the hair acts the part of a natural thatch. The external ear, which is in a more degenerate condition than the ear of any other primate, man included, has been described or figured by Schwalbe (275*a*), Barkow (90), Beddard (93), and Trinchese (283). Most of the authors I have cited in this section give measurements of the length of limbs and trunk, but Rollet (186*a*), Wallace (284, 285), and Blyth (224) enter most fully into this matter.

Distribution.—The orang, as is well known, is confined to the islands of Borneo and Sumatra, and to certain parts only of these islands. Its distribution is limited to the swampy mangrove forests along the coast, especially along the estuaries of rivers. It is most abundant in Borneo. Its exact distribution in that island has never been worked out, but the great majority of specimens come from the south-west corner, especially along the Kapuas River—*see* Selenka (277), Wallace (284, 285), Brooke (226), Hartmann (43), Hornaday (247), Mohnike (260), Schlegel and Müller (272), Rosenberg (267), and Trinchese (283). The orang is a much rarer animal in Sumatra, few specimens ever coming from there, and is confined chiefly to the north end of the island. Our information, however, is limited, what we have being due to Abel (217), Rosenberg (267), Veth (279), Wenckstern (286), and Schlegel and Müller (272).

Pathology.—Owen (261, 263) records the lesions found in two animals; Rollet (186) and Topinard (282) found cases of bone disease;

Schmidt (195) deals with their diseases in captivity. Of two young animals that I kept in captivity while in Siam, one died of ulcer of the stomach, the other of pneumonia, both lesions being exactly of the type found in corresponding human diseases. The subcutaneous tissue of these two animals contained a large number of thread-worms belonging to several species.

Classification.—It cannot be said even yet that the question as to the number of orang species is finally settled, but all the evidence at our disposal leads to the conclusion that there is but one species. This has been the opinion of Dutch naturalists all along. The great amount of structural variation—in the shape of the skull, presence or absence of temporal crests, of cheek-pads, or of great toe-nails, in the form of the teeth or colour of the hair, which was by some regarded as indicating a difference of species, is now generally looked upon as due to individual age or sex peculiarities. Brooke (226), Owen (262, 264), Blyth (224), and Wallace (284, 285) distinguished several species, but as pointed out in the section on the cranium, the specific differences were really age characters. Selenka (277), who more than anyone has had the advantage of studying abundance of material as well as the animal in its native haunts, regards the orang as forming one species, but distinguishes six local varieties in Borneo and two in Sumatra. From the characters he assigns to these, however, it may be doubted if these varieties could be distinguished from each other with any certainty. Lucas (253), who studied the material brought home by Hornaday, came to the conclusion there was but one species, and Temminck (280), Schlegel (193), Schlegel and Müller (273), Milne Edwards (258), Beddard (93), Brühl (227), Dumortier (232, 233), and Lucae (252) were of a similar opinion. Giglioli (31) distinguishes two species of Bornean orang, Fitzinger (236) distinguished four, while Gray (134) gives a full list of the species, with their characters, enumerated in his time. It is generally held that no line of demarcation can be drawn between the orang of Borneo and that of Sumatra, but our knowledge of the Sumatran animal is extremely scanty. I was able to find in London only five skulls of the Sumatran orang, and, working over these very minutely, could find no mark to distinguish them from Bornean skulls. Still, a further study of the anatomy of the whole animal might reveal permanent points of difference. Our knowledge of the Sumatran orang is due principally to Abel (217), Möbius (259*a*), Snelleman (279), Schlegel and Müller (273), and Wenckstern (286), who distinguishes two kinds of Sumatran orangs.

There is a concurrence of opinion in regarding the orang as much more closely allied to the gorilla and chimpanzee, than to the gibbon on the one hand or to man on the other. Meyer even proposed to place the three in one genus. There has been a great deal written upon the affinities of this genus, principally by Broca (104*a*), Duvernoy (22), Geoffroy St. Hilaire (239), Owen (264, 175), Huxley (49*b*), Mivart (61*a*), and Hartmann (138, 43).

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ARTHUR KEITH.

576.3

V.

The Cell, and some of its Supposed Structures.[1]

IN endeavouring to arrive at a correct appreciation of the status of the multitude of cell constituents that our improved methods of technique have revealed to us, we find ourselves in a position of no inconsiderable difficulty. A few years ago we believed we possessed a good deal more knowledge on these matters than we feel we can lay claim to at the present time. This change is to be attributed partly to the healthy attitude of criticism which cytologists have assumed towards the methods they employ, but still more to the conviction, which is ever being strengthened, that the earlier researches, classical though some of them have deservedly become, neither indicate the limits of the subject nor can be regarded as representing generalised type-forms.

Seldom has the need for caution been more effectively enforced than in the history of the so-called "quadrille of the centrosomes," first described by Fol, as occurring during the fertilisation of echinoderm eggs. Most biologists hailed this discovery with joy, which was greatly increased when the announcement was made by Guignard that he had observed a corresponding condition in the lily. Nevertheless, at any rate, so far as the animals are concerned, the foundations on which the whole observations were based have been destroyed; all recent competent investigators are agreed that Fol was mistaken in his conclusions, and that his mistake arose from the defective fixation of his material.

The centrosome question, on which so much has been written during the last decade, is in a very unsatisfactory condition. Few investigators are agreed as to the actual nature of this body, and perhaps fewer still as to the part which it plays in the cell. Some regard it as the active agent in bringing about nuclear division, while others believe it to be a transient structure, called into existence by

[1] Short abstract of a paper read before a combined meeting of Sections D and K at the meeting of the British Association at Liverpool. The full text will appear later in the *Annals of Botany*.

We beg to remind our readers that the terms used and the theories criticised in this article have already been explained fully in NATURAL SCIENCE: *see* M. D. HILL, "Cell-division," vol. iv., p. 38, Jan., 1894, and p. 417, June, 1894; and RUDOLF BEER, "The Nucleolus," vol. vii., p. 185, Sept., 1895.—ED. NAT. SCI.

the forces which are operating in the cell, and which finally produce the phenomena of karyokinesis; and the extreme advocates of these respective views only weaken their case, as well as cause a difficult subject to become still more obscure, by refusing to look at the other side of the question. For, while it is certain that in some cells the centrosome does possess a marked individuality of existence, it must be remembered that this fact is not one of *general* application; hence it is absurd to speak of it as a "necessary organ" of division. The recent studies of Hertwig, for example, on the behaviour of unfertilised echinoderm eggs, as well as the observations of botanists, have shown that nuclear division may be initiated and even completely gone through without the occurrence of a visible centrosome; and further, in certain other cases (*e.g.*, spore-mother-cells of liverworts), where these bodies are present there is absolutely no proof that they are anything but new and temporary formations in the cell.

In fact, there is but shadowy evidence, even in the most favourable instances, that the centrosomes act as principals in the process of karyokinesis; they may well only be aggregations which mark the centres of the forces operating within the protoplasm. They would occupy a similar *position* whether they were the active agents, or merely the passive insertion points of the radiations. We know that contractility is a property of protoplasm, and there is no *à priori* need for assuming a special organ to direct the procedure of that special kind of contractility which results in the partition of the nucleus. *Post hoc* does not always mean *propter hoc*.

But these considerations do not affect the further conclusion, that a certain structure may be produced by a definite interaction of forces upon matter, and that in the case of protoplasm this might lead to an aggregation of such a character as that to which we give the name of centrosome; and that, further, the same set of physical and mechanical conditions being periodically reproduced, would lead to as regular a reappearance of the structure—the centrosome. From what we know of protoplasm it would not be difficult to imagine that something like this does go on in cells, and possibly this may account for the periodical reappearance, and even permanence, of these bodies in some of the cases in which they have been observed. Still, it must be remembered that we actually *know* so little about protoplasm that any such line of thought is merely speculative, although it may serve for the present to link together the apparently irreconcilable results which have been reached by different investigators.

The present position of the question as to the origin and nature of the achromatic spindle is also in a very unsatisfactory state. Does the spindle arise as the result of an onward development of a pre-existing rudiment, or is it a new formation in the protoplasm? In the answer to this question, no less than in the conclusion to which

we arrive respecting the nature of the centrosome, an important principle is involved. It is doubtless simpler to admit the existence of a variety of 'organs' in the cell; but does such an admission bring us any nearer to understanding the actual processes of cell-life? Looking the facts squarely in the face, it is very difficult to maintain the hypothesis of an independent autonomous development for the spindle. Most of the carefully worked out results of recent years seem to point to it as arising as a differentiation out of the normal protoplasmic structure, and not as the replacing of this by an intrusive centrifugal outgrowth of a special substance surrounding the centrosome. But, just as with this latter body, so also with the spindle, there are differences exhibited by different cells in the degree of mechanical complexity and completeness characterising their spindles. These differences may well be, and, indeed, fundamentally must be, due to physical differences existing between the different protoplasms; thus the ultimate extent of delicacy of adjustment adapted to meet the individual requirements in any given instance would come to depend on the constitution of the protoplasm itself. The same forces acting through different mechanisms will naturally produce dissimilar results.

Again, the chromosomes themselves present abundant difficulties, when one tries to form a rational conception which shall embrace the facts now known respecting them. If individuality and permanence be conceded to the chromosomes, how can this be reconciled with the facts of reduction and of fertilisation? It seems perfectly clear that the reduction, which, so far as is known, always occurs at some point before fertilisation, can be effected in various and radically different ways. And if this is so, how can one grant even the probability of the existence of that complex structure which has been ascribed to the chromosomes as the supposed bearers of the hereditary qualities of the organism?

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SOME NEW BOOKS.

Semon in the Australian Bush and on the Coasts of the Coral-Sea.

Im australischen Busch und an den Küsten des Korallenmeeres. Reiserlebnisse und Beobachtungen eines Naturforschers in Australien, Neu-Guinea und den Molukken. Von Richard Semon, Professor in Jena. 1 vol., 8vo, pp. 570. Leipzig: Engelman, 1896.

Books about Australia are numerous enough, but are mostly devoted to a monotonous record of journeys in a barren and thirsty land where no water is. Among the few that vary from this general character we can only call to mind Lumholtz's "Among Cannibals," which contains a most interesting narrative of the author's adventures with the native tribes of Northern Queensland and of the animals of that district. But the Swedish naturalist must now give way to the German professor, who has not only made remarkable discoveries in the course of his wanderings, but evidently knows well how to bring them before the public. Professor Semon, who dedicates his narrative to Professor Haeckel and Dr. Paul v. Ritter, two of his brother professors in Jena, left his university in June, 1891, with the principal object of investigating the three great wonders of the Australian fauna—the egg-laying mammals, the marsupials, and the lung-fishes. In all these objects he seems to have met with undoubted success. Proceeding straight to Australia, by Aden and Ceylon, he found himself in a few weeks at Brisbane, and after a short stay took up his quarters in the backwoods of the Burnett district of Queensland, traversed by the Burnett and Mary Rivers. In these streams alone the "living fossil," as our Professor calls the *Ceratodus*, has as yet been ascertained to exist, though in former days it was doubtless met with in other Australian rivers. Professor Semon, although, as will be seen from many passages in his narrative, a most enthusiastic supporter of the Fatherland and its interests all over the globe, appears to have much appreciated the good qualities of the English residents with whom he made friends in Queensland, and is unstinted in his thanks for their assistance and in his praises of their hospitality. Altogether he passed the greater part of two years in the wilds of this district, which is not only the home of *Ceratodus*, but also of the monotremes and marsupials of which he was especially in search. But Professor Semon by no means confined his attention to these subjects; he made huge collections in, apparently, every branch of natural history. Nor did he by any means neglect the wild man of Australia, without whose aid he would have had very little success in his investigations. Our author devotes his tenth chapter wholly to an elaborate account of the habits and manners of the Australian natives, and dilates upon every particular as regards their customs, history, and supposed alliances to other native races.

The second point in the Australian Continent investigated by Professor Semon, was in the coast-ranges of Northern Queensland, of which he gives a lively sketch. Here our traveller seems to have been attracted by the existence of a tree-kangaroo (*Dendrolagus*

lumholtzi), which is only found in this part of Australia. As regards this animal, which appears to be both scarce and shy, our author was not so successful as on the former expeditions; still, he met with a different set of marsupials in this district and with many other novelties, including numerous crocodiles and so-called flying foxes (*Pteropus*).

Leaving Australia on his way northwards, Professor Semon proceeded to Thursday Island in Torres Strait, where he cruised about and inspected the pearl and trepang fisheries and investigated the marine fauna. Here, too, he made a most interesting excursion to the southern coast of New Guinea, and introduces a vivid account of the native Papuans, with some good illustrations of their faces and their dwelling-places. Proceeding up the Gara River he had the pleasure of shooting some paradise-birds (*Paradisea raggiana* and *Cicinurus regius*), and of obtaining specimens of other products of this strange land. An excellent chapter is devoted to a disquisition on the Papuan race and its alliances.

From Thursday Island, after returning to Australia for five months in order to make his collection of the various stages of *Ceratodus* more complete, our traveller proceeded to Java, where, like all other visitors, he was enchanted with the beauty of the celebrated botanic garden at Buitenzorg and the luxuriant vegetation of the whole island. From Java Professor Semon made a long excursion to the northern Moluccas, travelling by Celebes to Amboina, where he secured considerable collections both of marine and terrestrial fauna. From here, after a short visit to the Banda group, Professor Semon returned to Europe by Calcutta, whence he did not omit to run up to Darjeeling and see the celebrated view of Kinchinjunga and the Himalayas. We must allow that he laid out his time well, and did not fail to take every opportunity of seeing all the sights that Nature could afford him.

In conclusion, we can truly say that Professor Semon's narrative is one of the best put together and most instructive volumes of travels that we have ever read, rivalling even the well-known "Naturalist on the Amazons," of Bates, and the "Malay Archipelago," of Wallace. Since we read "The Cruise of the 'Marcheesa,'" no journal of a natural history expedition has interested us more. Professor Semon's volume is, moreover, abundantly illustrated from well-executed photographs. Unfortunately for our countrymen, many of whom are still imperfectly educated, Professor Semon has chosen to write it in his mother-tongue, but we are sure that an English translation of this excellent piece of work would be much appreciated, and trust that measures will be taken to set it before the British public in their own language.

P. L. S.

THE CAUSE OF MALARIA.

ROMAN FEVER: the results of an enquiry, during three years' residence on the spot, into the Origin, History, Distribution, and Nature of the Malarial Fevers of the Roman Campagna, with especial reference to their supposed connection with Pathogenic Organisms. By W. North. 8vo, pp. xx., 419. London: Sampson Low, Marston & Co., Ltd., 1896.

THE GOULSTONIAN LECTURES ON THE LIFE-HISTORY OF THE MALARIA GERM OUTSIDE THE HUMAN BODY. Delivered before the Royal College of Physicians of London, by P. Manson, March, 1896. Reprinted from *British Medical Journal*.

IN an article published in NATURAL SCIENCE in September, 1894, an account was given of the researches made up to that date as to the cause

of malaria. The case was there stated for the belief that this malady is due to the presence of a parasite, which attacks the corpuscles of the blood; the life-history of this parasite was summarised so far as it was known, its biological affinities were indicated, and the action of quinine upon it was explained. Having given space for a definite exposition of the parasitic theory, we are bound to call attention to a new work, in which the case for the opposite view is stated in great detail.

The volume is the work of a man who has devoted much time and trouble to the study of this subject. As a research student for the Grocers' Company he went to Italy in 1883, and spent three years investigating the disease and the conditions under which it flourishes. He worked in the medical laboratories of Rome; he studied the geology, hydrology, and meteorology of the infected areas; he treated cases medically, and noted the effects of food and drugs on the course of the disease. He seems to have spared no pains, and with the courage of his profession never hesitated at serious personal risks.

His book is divided into three parts, dealing respectively with the distribution of malaria, with the meteorological and other factors which control it, and with the phenomena of the disease. The author first dwells on the great importance of the subject, illustrating his remarks by the facts that in India half the military forces are temporarily disabled by disease every year, and that the expense of the administration of that country is greatly increased by the lessened efficiency of individuals. The next two chapters state the distribution of malaria in the world, and especially in Italy. The author then treats of the Roman Campagna in detail, describing its physical geography, geology, and hydrology, and the history of malaria in the province from the earliest records downward. In the second part he gives the result of his investigation into the conditions under which malarial fevers exist, and the circumstances which modify them. He claims that "the only relation of malaria to altitude consists in the diminution of temperature which accompanies the latter" (p. 99), and points out the intimate relationship between malaria and wet soils (p. 109). He attaches great importance to the effect of subsoil water, and maintains that "the intensity of the disease increases as the subsoil water approaches the surface." He refers to localities such as the Amazon Valley, which he accepts as non-malarial, although, according to popular ideas, they ought to be infested with the disease.

Considerations such as these lead to the argument in the third part of the book. The author there contends that the disease is the direct effect of sudden changes of temperature; he claims that the parasitic theory is "utterly unsatisfactory," and that the "chill theory" is sufficient to explain all the facts. He ingeniously argues that there is a normal periodicity in the human temperature, and that in malarial fever there is simply an exaggeration of this, produced by chills upsetting the normal relations of the heat-producing and heat-dissipating processes of the body. The liability to a recurrence he explains as due, not to hæmatozoa persisting in the marrow-cells or in the spleen, but to the patient having acquired, "from some unknown cause, a periodicity of function."

This third part of the book is the least satisfactory; the author does not disturb our faith in the parasitic theory, nor does he bring the chill theory into harmony with all the known facts or render it capable of explaining the phenomena of the disease. To take, for example, one illustration of the effects of violent changes of tempera-

ture. The temperature of Zanzibar is one of the most uniform in the world; the annual range of temperature is about 70° Fahr., but the town reeks with malaria. On the other hand, on parts of the Masai plateaux the daily range of temperature may be 70°, but malaria, in some of these places, is unknown.

The author's criticisms of the organic theory are not always up to date. He shows a full knowledge of the literature of his subject up to 1885, but not for later years. His arguments are often fatal to the organic theory of 1886, while they do not shake that theory as we know it in 1896. It is within the last six years that the hæmatozoic theory has been proved. Pages are devoted to the refutation of the suggestions of Salisbury, Klebs, and Tommasi-Crudeli, which have now only an historic interest, whereas the discoveries of the last few years are not considered. The author is clearly a patient and conscientious worker, who mastered the whole subject as far as was possible at the time of his three years' research upon it. Had the book been published immediately after his return it would have been of great value; but since then the author does not seem to have had his former opportunities or access to medical literature. Hence the book is several years out of date, and its main value depends on its account of the past history and present distribution of the disease.

Dr. Manson's Goulstonian lectures treat malaria from an altogether different point of view. He accepts the parasitic theory as fully established. He tells us that "there cannot any longer be the slightest doubt that it [Laveran's parasite] is the germ of malaria; there cannot any longer be the slightest doubt that incidents connected with its multiplication in the human body are the causes of the clinical phenomena of malaria. These things are now abundantly proved, and are accepted by all who have taken the trouble to make themselves personally acquainted with the facts and the parasite." Starting from this premise, his faith in which we do not think at all excessive, Dr. Manson endeavours to trace the life-history of the malarial hæmatozoon during the stages which it must pass through outside the human body. It was known that when malarially-infected blood corpuscles were drawn from the body some of the malarial spherules broke up into free-swimming flagellated bodies. The explanation given of these flagellated bodies by most workers is that they are degenerate hæmatozoa (or plasmodia, as they are generally called in medical literature). This is the view adopted by Blanchard, Labbé, Marchiafava, and Begnàmé, but opposed by Laveran, Nannaberg, Danilewsky, and others. Dr. Manson now explains this flagellated stage as one adopted by the hæmatozoon when it escapes from a body in order to secure its proper distribution. He considers that it is the first extra-corporeal stage in development, and, as it cannot be produced in the host, he concludes that one part of the normal development of the hæmatozoon is outside the body of the host. He suggests that the mosquito is the agent which effects the liberation of the parasite. He shows that this is probable from the facts of the distribution of the disease, and from the interesting analogy between filaria and the malarial parasite. This evidence is not conclusive, but some experiments made in India by Surgeon-Major Ross at Dr. Manson's suggestion have certainly made one important step towards proving the theory, for it has been demonstrated that if a mosquito sucks in blood containing the malarial spherules, that these develop in the insect into the flagellated bodies.

Considering the enormous influence of malaria in all tropical

countries, it is greatly to be hoped that Dr. Manson's theory will soon be further tested. For if it be proved that mosquitoes are essential to one stage of the life history of the parasite, sanitary engineers will know one method by which its ravages may be checked.

JURASSIC BRYOZOA.

CATALOGUE OF THE FOSSIL BRYOZOA IN THE BRITISH MUSEUM (NATURAL HISTORY). The Jurassic Bryozoa. By J. W. Gregory. Pp. 239, pls. xi. Printed by order of the Trustees. London, 1896. Price 10s.

IN the catalogue before us the diagnosis is given of seventy-seven species of Cyclostomata, and of two Chilostomata. We are quite accustomed to authors prefacing descriptions of Cyclostomata by a note of despair as to the difficulty of classification: Dr. Gregory shows how many groups of invertebrates live in tubular shells, and admits that there are no diagnostic characters by which we can positively say whether a given fossil be bryozoan or not. The difficulty of this decision is greatest with the Paleozoic Bryozoa, but even Bryozoa from the Jurassic formation have been placed among corals, while Hydrozoa such as *Acanthopora*, *Neuropora*, and *Chrysoaro* have now had to be removed.

Zoarial characters are considered to be the only ones available for systematic work on the Cyclostomata, and Gregory accepts the divisions based upon them, but shows how one genus passes into another, and asks the question as to whether there are any genera in Cyclostomata; answering that there are only certain convenient, but artificial, groups of species. The groups called "genera," Gregory would, in the Cyclostomata, prefer to call "circuli." Another way of putting our difficulty would be to say that, although there must be groups of species having relationships, which in other orders we should call generic, yet here, on account of the simplicity of the calcareous structure and few characters available, we are often unable to trace the relationship.

The extremest pessimist may, however, hope for some increase of knowledge; for example, the ovicell, which was not formerly noticed, is now always keenly looked for, though the number of species on which it has not yet been found is very large. Out of the seventy-five species of Cyclostomata diagnosed by Gregory, he only mentions the ovicells in twelve cases. No doubt by the discovery of further specimens, and by making sections, this number will be somewhat increased.

Useful as the ovicells are specifically, caution is required; for a character may in some cases have generic value, in others specific, or the same character may have no importance, and, until a large number are known, it is unsafe to make generalisations. We see the danger in Gregory's attempt to use the position of the ovicell as a family character in the Chilostomata, for in his "British Palæogene Bryozoa," and in the present catalogue, he makes both the Membraniporidæ and the Microporidæ have "external oœcia," which in neither case is universal. In the subfamily Membraniporinæ it is certainly usual, though the best known of the Membraniporidæ, *M. pilosa*, has recently been removed to *Electra*; and Norman, describing the genus, writes "no oœcia known in recent species," and says that he thinks a fossil described by Waters as *M. Lacroixii*, var. *grandis*, from New Zealand, must, on account of its possessing an external ovicell, be removed to another genus. This is apparently going too far, as the writer has a specimen of *M. Lacroixii* from Naples with a small raised ovicell, and it seems that in Savigny's plate the ovicell is figured, though the artist has drawn it incorrectly, placing it below the zoœcium.

Jullien made a "tribus inovicellata" for the Aetidæ, but a paper is now in the hands of the Linnean Society figuring the ovicells.

In the Microporidæ there are a large number of species with external ovicells, while many are without; but is Gregory right in placing *Onychocella* in this family? The genus *Onychocella* is a very interesting one, occurring in great abundance in the Chalk, continuing abundant through the Tertiaries, with only one or two representatives now living. The form described by Gregory as *O. flabelliformis*, Lamx., very closely resembles several Chalk species, but revision of these is much wanted, as the number of synonyms is known to be very great. We may, however, hope to see this genus brought into order when the catalogue of the Cretaceous Bryozoa is published.

In *Micropora* the oral aperture corresponds with the operculum, but this is not the case in either *Membranipora* or *Onychocella*. In the living *Onychocella angulosa* the opesial opening is large, while the operculum is but small, and is entirely attached to the membranous wall. Further, there are no external ovicells, and the ova pass into a membranous chamber at the distal end of the zoœcium, near to the basal wall. In recent species of *Onychocella* there are trabeculæ bordering the operculum as in *Cellaria*, and an interesting Cretaceous fossil called *Escharella argus*, d'Orb., has teeth in the aperture like *Cellaria*, and has avicularia of the type known in *Onychocella*, while the ovicells are like those of *Cellaria*. In the fossil *Onychocella* an external ovicell is rare, though in *Onychocella lamarckii* there is a slight elevation where the "oberhöhle" of Hagenow occur; and Jullien speaks of the ovicell of the Onychocellidæ being raised, but in none of the species described by him is there any ovicell. In a number of forms such as *Eschara dichotoma*, Hagenow, it is very difficult to decide whether the opening is opesial, or was closed by the operculum.

To return to Dr. Gregory, he accepts Ulrich's order Treptostomata, which contains the families Ceramoporidæ, Heterotrypidæ, and Amplexoporidæ, represented in the Jurassic by *Ceriopora*, *Chilopora*, and *Heteropora*, but the *Heteropora* of Blainville is somewhat limited.

The Cyclostomata are divided into four suborders, the Articulata, Tubulata, Dactylethrata, and Cancellata. "Dactylethræ" is the name proposed for a form of "aborted zoœcia" consisting of short cæcal tubes closed externally, but whether this is, as Gregory considers, a structure of classificatory value, may at present be considered an open question; however, much of the usefulness of such a work is that questions are raised which lead to further investigation. The best critic of classification is time, but many groupings which have not stood this test have yet been useful in helping towards an orderly arrangement of our knowledge.

Since the genus *Meliceritites* included species with well developed avicularia, and others distinctly cyclostomatous, Gregory has placed the latter in a genus *Haploœcia*.

This catalogue, clearly placing before us what is known concerning the Bryozoa of the Jurassic period, will be warmly welcomed by those who are working at this or kindred subjects, and will be a standard work of reference. A. W. W.

The Literature of the Pleistocene in Ireland.

A Bibliography of Irish Glacial and Post-Glacial Geology. By R. Lloyd Praeger. Being an Appendix (no. 6 of vol. ii.) to the *Proceedings of the Belfast Naturalists' Field Club* for 1895–96. Pp. 239–316. 8vo.

The literature referring to the Glacial and Post-Glacial geology of Ireland is somewhat scattered, and many valuable papers occur in

publications that are not in the hands of the ordinary geologist, such as the *Transactions* of the Kilkenny Archæological Society or the *Journal* of the Historical and Archæological Association of Ireland. In the absence of any bibliography, the work of the numerous energetic naturalists who are springing up all over Ireland in connection with the various field-clubs, to which we have often alluded, was necessarily hampered, and this careful compilation by Mr. Lloyd Praeger will be welcome specially to them and also to students of Glacial phenomena in other countries. The bibliography includes papers not only on the Pleistocene beds of Ireland, but also on the "manure" gravels of Wexford, which, though generally considered to be of Pleiocene age, possess a fauna related to that of the Glacial period. References are also included to the literature of the caves, on which so much valuable work has recently been done in Ireland. The bibliography appears to have been carefully compiled, and is clearly printed. It might perhaps have been improved from a technical point of view had some indication been given when the name of an author was carried over from one page to another. For instance, the reader seeing **90 Bell (Alfred)** at the top of page 252 might suppose that No. 90 was the first paper by Alfred Bell recorded in the book, whereas the papers ascribed to him really begin at No. 85 overleaf. The difficulty is easily got over by the insertion of "*cont.*"

The value of the list is increased by an index to the numbers under various geological, palæontological, and geographical heads, such as "raised beaches," "prehistoric implements," "Co. Cork." This index, as the author himself recognises, would have been of far greater value had it been compiled from the papers themselves, and not merely from their titles or from the brief indication of their contents that has occasionally been added by Mr. Praeger. Authors are too fond of giving to their papers titles that convey a totally wrong impression of the contents, and abstractors are too prone to pay attention to the points that interest themselves rather than others, for such a method of constructing an index ever to produce a reliable and final guide.

The copy sent to us for review is in printed wrappers with a separate title-page, but it does not appear whether the work can be purchased apart from the *Proceedings* of the Belfast Field Club. We hope it can.

TYNDALL'S GLACIERS.

THE GLACIERS OF THE ALPS: being a Narrative of Excursions and Ascents, an Account of the Origin and Phenomena of Glaciers, and an Exposition of the Physical Principles to which they are related. By John Tyndall. New Edition. 8vo. Pp. xxvii., 445, with 5 plates aud 57 figures. London: Longmans, Green and Co., 1896.

THE recent growth of the interest taken in Alpine gymnastics has in one way been a source of inconvenience to geologists. During the last few years there has been so great a demand for the earlier Alpine literature, that most of the best books have long since gone out of print, and risen to a fancy value that places them out of the reach of most naturalists. In the case of books of purely topographic interest, this is not a matter of much importance; but when fashion increases the cost of such an important work as Tyndall's "Glaciers of the Alps" to many times its published price, it is decidedly matter for regret. Glacial geologists will therefore be very grateful to Mrs. Tyndall for her republication of this book. Beyond welcoming its appearance we need say little: the book is too well known. It is a

glacial classic, and no one interested in the long controversy on the nature of glaciers, and the cause of their movements, can afford to ignore it. It is true that most of the second part of the volume was re-issued in an abridged and somewhat more popular form, in Tyndall's "Forms of Water"; but for advanced students, the original and longer discussion of the subject is to be preferred. The accounts of Tyndall's Alpine ascents, given in the first part of the book, are of great interest, though, perhaps, not equal to the author's "Hours of Exercise in the Alps." The present edition has a greatly improved index, but otherwise Mrs. Tyndall has wisely left the text unaltered. A short introductory chapter on the advances made in the subject since the first edition would have been extremely useful, as the volume will, no doubt, find its way into the hands of many visitors to Switzerland who know nothing of later researches.

Plankton Pelecypods.

Die Acephalen der Plankton-Expedition (Ergebnisse der...Plankton-Expedition der Humboldt Stiftung, etc. Bd. ii., F.e.). Von Dr. H. Simroth. Pp. 44, 3 pls. Kiel and Leipzig, 1896.

We have on a previous occasion (vol. viii., p. 343) called attention to the section of this work by the same distinguished author, treating of the gastropod forms, mostly larval, met with on the Plankton Expedition; the present part, though much smaller, is conducted on the same lines and with the same care.

With one exception the Pelecypoda prove to be larval forms belonging to taxodont, desmodont, and probably other groups. Most of them had passed the velar stage, and were dimyarian and integropallial. The greater number were probably pelagic only by accident, and exhibited no special adaptation for the life. Nor did the only perfectly formed bivalve met with, *Planktomya henseni*, n.gen. et sp., though its foot was largely aborted. This truly pelagic little mollusc is only from ·5 to ·75 mm. in length, and occurs in the warmer regions of the Atlantic. Its shell appears to be destitute of lime.

Prefixed to the paper, which contains many points of great interest not limited to its subject matter, is a chapter, with a table, on the development of the different groups of Mollusca under different conditions of environment. Among other forms tabulated one is surprised to find that mythical monster, the 'hypothetical primitive mollusc,' and to learn that it dwelt between tide-marks and in the littoral zone, while its larval form was hemipelagic.

New Serials.

It is a genuine pleasure to observe how steadily the study of Ornithology is gaining ground in the United States. *The Osprey* is the youngest of American magazines, but it has left the eyrie under happy auspices. It is edited by Walter A. Johnson and Dr. A. C. Murchison, and published at 217 Main Street, Galesburg, Ill. The papers contributed to the first number, issued in September last, embrace a variety of subjects, including a paper on Terns, an admirable essay on the "Nesting of the Ferruginous Buzzard," and other reports of useful field-work. An interesting feature of this journal is to be found in the picturesque photographs which are reproduced in its pages. We are glad to observe that *The Osprey* "is not a money-making scheme, nor wholly dependent upon the first year's receipts." We congratulate the editors upon the plucky start which they have accomplished, and trust that their venture will receive wide support.

Western Field and Stream (St. Paul, Minn., U.S.A.) is another new journal that has been sent to us. It is copiously illustrated, and though devoted chiefly to sport, contains one or two articles of interest to naturalists. The subscription is one dollar a year.

A new quarterly, *Revista Trimestrial Micrografica*, is announced, under the editorship of Dr. Ramon y Cajal, Professor of Histology and Anatomy in Madrid University.

From October 1 *Science Progress* changed into a quarterly review of current scientific investigation, at an increase in price to 3s., and in size to an average of 110 pages. The solid value of its contents undoubtedly adapts them for the dignity of a quarterly.

The Nordoberfränkischer Verein für Natur- Geschichts- und Landeskunde, which was founded at Hof, Bavaria, in 1891, has now begun to publish a *Bericht*. No. I., which is an octavo of xiv. and 110 pages, with two folding plates, is said to have been delivered in July, but we saw no copy in this country before the end of September. One of the plates represents a section along the railway at Neuhof station near Hof, where fossiliferous beds of Tremadoc age have lately been exposed. The fossils are described by Dr. J. F. Pompeckj, who records the occurrence of eleven species already described by Barronde in his "Faune silurienne des environs de Hof," 1868, and of three new but still unnamed species belonging to *Lingula*, *Acrotreta*, and *Bellerophon*. To this number also Dr. Ernst Zimmermann contributes an article, "Ueber Wesen und Ziele der geologischen Landesaufnahmen," and another entitled "Einiges ueber die Loessfrage."

VARIA.

Two parts of the eight large volumes to be issued by Gustav Fischer, under the title, *Handbuch der Anatomie des Menschen*, have appeared, edited by Professor von Bardeleben. The first part, by Professor J. Disse, deals mainly with the spinal column.

Science for September 4 contains an interesting article on Botanical Gardens, their origin and development, by N. L. Britton, with notes on the chief Gardens in Europe and in the United States. An appreciation of Dr. P. L. Sclater, by the late Dr. Brown Goode, is also to be found in this number.

The National Museum at Washington has been benefiting for some eight years by the addition of collections made in different parts of the world by Dr. W. L. Abbott. This explorer has sent material from East Africa, the Seychelles, Cashmere, Turkestan, and Ladak, which is being worked out by the staff of the Museum; two reports on birds have already been issued.

FURTHER LITERATURE RECEIVED.

Biology Lectures, Wood's Holl Lab.: Ginn & Co. How to study wild Flowers, G. Henslow: Religious Tract Soc. Handbook of Physiology, W. D. Halliburton: Murray. Testimony of Science to the Deluge, W. B. Galloway: Sampson Low. Die Morphologie und Physiologie der Pflanzlichen Zellkernes, A. Zimmermann; Die Bedingungen der Fortpflanzung bei einigen Algen und Pilzen, G. Klebs: Fischer. Certain Problems of Vertebrate Embryology, J. Beard: Fischer. The Priceless Gem, Balmokand: Mitra Vilasa Press, Lahore.

Voles and Lemmings, G. S. Miller: U.S. Dept. Agric. On Structure and Reproduction of Cystopus candidus, Lev., H. Wager: *Ann. Bot.* Reptilian Order Cotylosauria, On Pleistocene Mammalia from Petite Anse, 4th Contribution Marine Fauna of Miocene Period of U.S., 2nd Contribution to History of Cotylosauria, 6th Contribution to Marine Miocene Fauna of U.S.; E. D. Cope: *Trans. Amer. Phil. Soc.* Report, Jamaica Inst. Guide, Bristol Museum. The Yolk-Sac, etc., in Scyllium and Lepidosteus, J. Beard, *Anat. Anz.* *Trans. Norfolk and Norwich Soc.* vi., 2.

Nature, September 17, 24, October 1, 8. 15. Literary Digest, September 12, 19, 26, October 3, 10. Revue scientifique, September 19, 26, October 3, 10, 17. Irish Naturalist, October. Feuille des jeunes Naturalistes, October. Naturen, October. Nature Notes, October. Amer. Journ. Sci., October. Naturæ Novitates, 17 and 18. Science, September 11, 18, 25, October 2, 9. Scott. Geogr. Mag., October. Science Gossip, October. The Naturalist, October. Westminster Review, October. Botanical Gazette, September. Review of Reviews, October. Knowledge, October. Photogram, October. L'Anthropologie, September. Amer. Natural., October. Vict. Natural., August.

OBITUARY.

GEORGE BROWN GOODE.

BORN IN NEW ALBANY, IND., FEBRUARY 13, 1851. DIED AT WASHINGTON, D.C., SEPTEMBER 6, 1896.

ON the 7th of September, fifty years ago, the Regents of the Smithsonian Institution met for the first time; on the same day of the present year they were to have met and commemorated the event, while in our October number we had hoped to publish a special account of the Institution and especially of the National Museum, by the assistant secretary, the director of the museum, Dr. Brown Goode. But he who should have summoned the Regents died on the day before, and the pen that should have written for us had been laid down for the last time. We allude to this kindly promise to ourselves, for it was typical of the man. Eminent though he was as a scientific investigator, it was chiefly by helping others that he advanced science. After graduating in 1870 at the Wesleyan University, Middletown, Conn., he was led by Professor Baird to help the United States Fish Commission as a volunteer, and subsequently became officially connected with it. He was United States Commissioner to the Fisheries Exhibitions in Berlin (1880) and London (1883). Among his chief works in this connection were "The Game Fishes of the United States," 1879, "The Fisheries and Fishing Industries of the United States," 1884, "American Fishes," 1887, and, in co-operation with Dr. T. H. Bean, the great work recently published on Ocean Ichthyology, a treatise on the deep-sea and pelagic fishes of the world. It was in 1873 that Brown Goode became attached to the Smithsonian Institution, and an opportunity of showing his mettle came three years later, when, owing to the illness of Professor Baird, the charge of the Smithsonian Exhibit at the Philadelphia Exhibition (1876) devolved largely on him. The experience thus gained led to his being placed in charge of the national exhibits at the exhibitions in New Orleans, Cincinnati, Louisville, Chicago, and, quite recently, Atalanta. Here, and in the National Museum, of which he was made assistant director in 1881 and director, along with the assistant secretaryship to the Smithsonian, in 1888, his remarkable genius for museum organisation and administration was displayed. He led the revolution against the old dry-as-dust, lumber-room practice of museum arrangement, and the principle that guided him was thus expressed: "The exhibition of the future will be an exhibition of ideas rather than of objects, and nothing will be deemed worthy of

admission to its halls which has not some living, inspiring thought behind it, and which is not capable of teaching some valuable lesson." Fortunately for the world his enormous experience does not altogether die with him, for in their *Report* for 1895, the Museums Association printed his codification of "The Principles of Museum Administration," from which we cull a single aphorism. "A finished museum is a dead museum, and a dead museum is a useless museum." We wish that space permitted us to say more; but, after all, no further words could express our poignant regret at the loss of this man, snatched from us in the prime of life.

SIR FERDINAND VON MUELLER.

BORN 1825. DIED OCTOBER 9, 1896.

THIS eminent botanist, one of the most distinguished men of science of the southern hemisphere, was born at Rostock in Germany in 1825, and as a boy showed great fondness for botany. At the age of twenty-one he was told that his only chance of life was emigration to a warm climate. He accordingly sailed for Australia, and has lived there for the last fifty years. In 1852 he was appointed Government botanist of the colony of Victoria, and in that capacity, following the lead of Robert Brown, has done more than anybody for the development of Australian botany. In the first ten years of his residence in Australia he made many important and arduous botanical expeditions to various parts of what was then a comparatively little-known continent. He acted as botanist to Mr. A. C. Gregory's expedition of exploration in tropical Australia in the years 1855 and 1856, and was one of the first to ascend the Australian Alps, and to explore their rich stores of botanical treasure. In these extensive land journeys, to which Dr. (now Sir Joseph) Hooker refers in some detail in his essay on the Flora of Australia, he covered upwards of 20,000 miles.

Baron von Mueller was a most voluminous writer, his works, which deal almost exclusively with Australian botany, forming a library in themselves on its systematic and economic aspect.

In his many books and papers he dealt with both the recent and the fossil botany of the continent; his chief contributions to science may be here recalled:—Fragmenta Phytographiæ Australiæ (11 vols., 1858–1881); Eucalyptographia (1879–84); Index perfectus ad Caroli Linnæi Species Plantarum . . . 1753 (1880); Iconography of Australian Species of Acacia (1887–88); New Vegetable Fossils from the Australian Drifts (1874, etc.); Select Plants eligible for industrial culture or naturalisation in Victoria (1876); and "Select Extra-Tropical Plants readily eligible for industrial culture," which went through many editions, of which the ninth appeared in 1895.

When a general "Flora of Australia" was contemplated, Mueller, in the hope that the work would be entrusted to him, devoted his

utmost energies to collecting the necessary materials. But, situated in Melbourne, he was unable to do the very necessary work of comparison with the type-plants deposited in various European herbaria. The task was therefore allotted to George Bentham, who in his general preface to the work warmly recognises the invaluable assistance received from Mueller in the form both of notes and of dried specimens. This help is also acknowledged by the insertion of Mueller's name on the title-page.

One of his greatest services to economic botany was his indication of the value of the Australian gum-tree (*Eucalyptus*) in combating malarial fevers, and it was on his advice that the tree has been so extensively and efficaciously planted in many malaria-haunted districts of Southern Europe and America.

NICHOLAS RUEDINGER.

BORN MARCH 25, 1832. DIED AUGUST 24, 1896.

DR. RUEDINGER was born at Büdesheim, in Hesse, and studied medicine at Heidelberg and Giessen. He was appointed Prosector at the Anatomical Institute of Munich in 1855, and was presented to a professional chair at the University in 1880. He was successful in applying photography to the purpose of anatomical illustration, and invented a new method of preparing objects for use in anatomical and surgical instruction. Although best known as a human anatomist, he contributed to general anatomy a paper on the speech centre and on the brain of the apes. Ruedinger died at Tutzing, Bavaria, according to the *Daily Chronicle*, to which journal we are indebted for some of the above particulars.

THE August number of the *Alpine Journal* contains a sympathetic notice, by Dr. F. A. Forel, of the late Captain MARSHALL HALL, who was born in London, February 6, 1831, and died at Parkstone, Dorset, April 14, 1896. He was educated at Eton and Caius College, Cambridge, and served his apprenticeship in the Alps by making the first passage of the New Weissthor from Zermatt, in 1849. In 1867 he made the second traverse of the Aiguille de l'Eboulement, and from 1878 to 1884 he resided in Switzerland, making ascents of peaks and observations on the glaciers of the high Alps. He was one of the Alpine Club Committee for studying the oscillations of the glaciers in the British Empire and elsewhere, and in 1895 he initiated the formation of a committee of the International Geological Congress for the study of the variation of the glaciers of the world. On this latter subject Captain Marshall Hall contributed a paper to our pages in January, 1895. He was a frequent visitor to the meetings of the Geological Society and the Geologists' Association, and his keenness and insight will be much missed.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments are announced :—Professor Allbutt, to the Downing Professorship of Pharmacology, in Cambridge; J. C. Willis, Botanical Assistant in Glasgow University, to be Director of the Ceylon Botanical Gardens; Charles Elcock, to be Curator of the Art Gallery and Museum at the Free Public Library in Belfast, in place of J. F. Johnson, mysteriously disappeared; Mr. Stanley Flower, son of the Director of the British Museum (Natural History), to be Curator of the King of Siam's Museum at Bangkok; Dr. C. Winkler, to be Professor of Nervous and Mental Diseases at Amsterdam; Dr. E. Lesser, to be Extraordinary Professor of Dermatology at Berlin; Dr. Chermak, to the Chair of Comparative Anatomy and Embryology at Jurieff (Dorpat); Dr. Andreas Obezut, to be Professor of Anatomy, and Dr. Prus, to be Professor of Pathology, at Lemberg; Dr. Gräff, to be Assistant Professor of Mineralogy and Petrography in Freiburg University, Breslau; Dr. A. V. Fomin, to be Assistant at the Botanical Garden of Dorpat University; Dr. L. Montemartini and Dr. G. Pollacci to be Assistants, and Dr. F. Tognini to be Curator, at Pavia University Botanical Gardens; T. S. Hart, of Victoria, to be Lecturer on Geology and Botany at the Ballarat School of Mines; F. B. Loomis, to be Assistant in Biology at Amherst College, U.S.A.; Miss A. M. Claypole, to be Instructor in Zoology, and Miss J. Evans, in Botany, at Wellesley College, U.S.A.; R. E. Dodge, to be Associate Professor of Natural Science at the Teacher's College, New York; T. A. Williams, Professor of Botany in South Dakota, to be Assistant in the Division of Cereals, U.S. Dept. Agriculture; Miss Parker, of Alabama, to be Professor of Natural Sciences in the Georgia Industrial College at Milledgeville, U.S.A.; Dr. C. Hart Merriam, to be head of the newly-established Biological Survey of the Department of Agriculture, U.S.A.; C. P. Nott, of the Brown University, to be Assistant in Botany in California University; Dr. Ludwig Reh, of Sao Paolo, Brazil, to be Permanent Assistant to the Concilium Bibliographicum, Zurich.

MR. WILLIAM WHITAKER retires from the Geological Survey on October 22. Mr. Whitaker, who is senior officer, was appointed on April 1, 1857, and has therefore held service for nearly forty years. The good wishes of all geologists go with him in his retirement. His address is now "Freda, Campden Road, Croydon."

MR. W. W. WATTS, of H.M. Geological Survey, is *not*, as many of his friends have supposed, the W. W. Watts who has been appointed Chief of the Circulation Department at the South Kensington Museum.

THE 70th birthday of Professor Bastian, of the Imperial University and Ethnological Museum in Berlin, was the occasion for the presentation of his bust to the latter institution. A memorial volume containing thirty-two papers by leading German anthropologists, such as Dr. Boas, Dr. Seler, and Professor Ehrenreich, was presented to him.

THE establishment at Palermo of an International Botanical Station, under Professor Borzi, is proposed, and the co-operation of botanists is asked.

MRS. E. ROBY, Mr. E. A. Shedd, and Mr. C. B. Shedd, have offered the University of Chicago a large tract of land around Wolf Lake for the purpose of a lake biological station, and it is understood, says *Science*, that they will erect the buildings for the purpose if the offer is accepted.

In the Anatomical Building now being added to Chicago University will be placed the psychological laboratory, and the department of neurology under Professor Donaldson.

OVER half the amount required for a new science hall at Lake Erie Seminary, U.S.A., has been subscribed, as well as $10,000 for equipment.

THE Johns Hopkins University *Circular* for May, which is devoted to a general report of the courses of instruction, states that the geological library contains most of the leading journals dealing with geology, and has been enriched by the addition of the libraries of Professor G. H. Williams and Professor H. C. Lewis. Moreover, the Peabody library is always available for the use of students. A capital practice is that of bi-weekly meetings, for the more advanced students, at which the leading geological journals are reviewed. In botany the students have the good fortune to be allowed the use of Captain Donnell Smith's private herbarium, which contains some 78,000 sheets of specimens.

IT is proposed to erect, near Honolulu, a biological laboratory for the Hawaian Isles. According to the *Revue scientifique*, the cost, some £160,000, would be defrayed by Mr. C. R. Bishop, one of the richest bankers resident there.

PART of the late Professor A. M. Butljero's collection of butterflies has been presented to Moscow University, and the late Julius Flohr's collection of Mexican insects has been left to the Berlin Museum of Natural History.

A MUSEUM of natural history, geology, archæology, and technology is proposed for Hertfordshire, and a site near St. Albans has been offered by Lord Spencer, upon certain conditions. A sum of at least £5,000 must be raised for building and endowment, and the museum is to be in the hands of the County Council. We are glad to see that a curator is thought of as much importance as a building. A provisional committee has been appointed, including Lord Cowper, Sir John Evans, and the Hon. Walter Rothschild.

THE *Report* of the Manchester Museum for 1895-6, notes the importance of the recognition of the museum as a public institution by the Manchester City Council, in that a sum of £400 has been apportioned to the museum out of the Free Library Rate. The average Sunday attendance is 519, and may be considered highly satisfactory, seeing that the largest attendance ever recorded on a week day was 1,079. The increase in the collections and library is very marked. The arrangement of the minerals by Mr. Gilbert Rigg, under the supervision of Dr. Burghardt, has been completed as far as the end of the silicates, and it is hoped that a guide to this collection may shortly be published.

WHITECHAPEL rises superior to the nation in that the following evening lectures, with lantern illustrations, are being delivered in its Museum free of charge: on October 13, "The Egyptian Workman, Past and Present," by W. Flinders Petrie; on November 10, "Extinct Monsters," by the Rev. H. N. Hutchinson; on December 8, "The Meadows of the Sea," by G. R. Murray.

AN exhibition of the highest interest is being shown in the Muséum d'Histoire Naturelle, Paris. It comprises the collections of ethnography and natural history made by Mr. Pavie and his fellow-workers during their explorations in Indo-China.

THE Bradshaw Lecture at the Royal College of Surgeons will be delivered on November 5 by Dr. W. R. Gowers, and will be entitled "Subjective Sensations of Sound." Dr. Bastian and Dr. Luff are to be respectively Lumleian and Goulstonian lecturers for next year, while the Croonian Lecture of 1898 will be delivered by Professor Sidney Martin.

THE Tenth Congress of Russian Naturalists and Physicians will be held at Kiev, from August 21 to 30, 1897.

A SECOND national congress of the Mexican scientific societies will be held in Mexico City in July, 1897.

ON the South bank of the Lake of Sils, in the Engadine, an erratic block of granite has recently been erected, bearing the inscription, "In memory of the illustrious English writer and naturalist, Thomas Henry Huxley, who spent many summers at the Kursaal Hotel, Majola."

AN amount of nearly £2,000 (of which the Emperor of Russia contributed over £300) has now been raised by international subscription for the monument to Lavoiser proposed two years ago, and money is still coming in.

PROFESSOR SOLLAS, writing to the Royal Society, reports that the borings made in two places at Funafuti were unsuccessful, owing to a quicksand which was struck at a depth of about 70 feet and choked the borehole. But in the collection of fauna and flora good results were obtained, while a complete series of soundings was made by Captain Field.

AN expedition to Madagascar, under the auspices of the French Government, to be directed by Messrs. Milne Edwards and Grandidier, is being prepared at a cost of 600,000 francs. We wish them all success.

MESSRS. BARBIER intend to fit out a boat of 2,000 tons for an expedition to West Africa, to take back the negroes whom they brought to Paris and Rouen. The voyage there and back will occupy forty-five days: by way of Lisbon and Las Palmas, the boat will proceed to Dakar, Ste. Marie de Bathurst and Konakry. As *L'Anthropologie* points out, this is a splendid opportunity for anthropologists and naturalists.

THE plan elaborated by the amateur Arctic tripper, Mr. George J. Gould, of attacking the North Pole with the help of a cordon of depôts established a point further north from year to year, is thrown somewhat out of joint by the discovery of a deep Polar sea, with nothing more secure than floating masses of ice.

Julius von Payer's scheme for an Antarctic expedition seems to have met with some obstacles, and should these not be removed, the veteran explorer will start next summer for Franz Josef Land to meet Mr. Jackson. It is said that Mr. Harmsworth has offered his vessel to Von Payer for a new expedition.

The Peary Expedition has been unsuccessful in bringing back the large meteorite from Greenland, since the implements taken were not strong enough to lift the enormous weight of forty tons. Valuable collections and observations compensated for the disappointment, and another journey will probably be undertaken.

Some exceedingly valuable zoological results have been obtained by Commodore Wandel and his party in the Danish boat "Ingolf," which returned on August 27 from a two years' cruise in Icelandic waters.